Essays in Historical Economic Development

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The Faculty of Business, Economics and Informatics of the University of Zurich hereby authorizes the printing of this dissertation, without indicating an opinion of the views expressed in the work.

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Dissertation Overview

An emerging area of research within economics seeks to better understand contemporary economic outcomes by taking a historical perspective. This body of work has established that many of the differences in economic development that we see today have their roots in the past. A wide range of historical factors from all regions of the world has been demonstrated to have effects that are felt for decades, centuries, and in some cases even millennia. To better understand the specific causal mechanisms underlying the relationships between historical and contemporary factors, recent contributions to the literature have begun to draw from other disciplines, particularly history, anthropology, and psychology. For example, a number of studies have shown that historical events can affect the evolution of cultural traits, which are important determinants of contemporary economic development. While most of these studies have focused on documenting persistence and the long-term effects of historical shocks on culture, there is less evidence on the importance of shorter-run historical shocks.

My thesis addresses this gap by examining the role of short-run historical shocks in shaping individuals' culture and psychology. Moreover, it explores the importance of a specific cultural trait, which may have become more prevalent in response to conflict, for economic outcomes. In three separate chapters, the thesis presents evidence on a number of historical shocks from different parts of the world. The evidence suggests that cultural change can result from short-run negative shocks or political incentives among the elites. It also indicates that traditional beliefs in the existence of supernatural protection are significant enough to affect psychological well-being, economic decisions, and business growth. In the following, I briefly summarize each chapter's research question and main findings.

In Chapter 1, I investigate the effect of negative collective shocks on the importance of conformity to social norms. Norm conformity alters people's publicly displayed behavior, even when they privately hold different preferences or beliefs. While norm conformity is an integral part of human psychology, there is huge variation in the extent to which individuals conform to social norms between societies. According to cultural evolutionary theory, a key determinant is the experience of negative collective shocks. In this view, evolutionary selection favors psychological traits that promote social cohesion in response to shocks, including stronger conformity to existing social norms. Based on a stylized model, I test this hypothesis by linking occurrences of natural disasters and major economic downturns to large-scale survey data from around the world. The evidence shows that individuals who have experienced negative shocks place more importance in norm conformity today. Conversely, people who have lived through periods of higher economic growth put less emphasis on norm conformity.

In Chapter 2, which is joint work with Sebastian Ottinger, we study the effect of political threat on the spread of racial propaganda in the media in the late 19th century U.S. South. Racial inequality and racism are key determinants of contermporary poverty in the United States. Moreover, the literature has recently established that slavery and Jim Crow have persistent

effects that continue to be felt today. Building on these findings, we examine the case of the unexpected and short-lived electoral success of the pro-redistribution Populist Party in the 1892 presidential elections. The Populists sought support among poor farmers, regardless of race. This biracial alliance threatened the Democratic establishment in the South, providing it with an incentive to fan racial fears to split the newly formed coalition. Newspapers affiliated with the Democrats spread propaganda of attacks by Blacks on the White community, often involving allegations of rape. Using novel newspaper data, we identify these hate stories and show that they become more prevalent in the years following the 1892 presidential election in counties where the Populists were active. The effect is large and found in newspapers affiliated with the Democrats only. The evidence also suggests that the propaganda "worked": where newspapers spread more propaganda, the Democrats see stronger gains in presidential elections in the following decades, long after the Populists left the political arena.

Finally, in Chapter 3, which is joint work with Raul Sanchez de la Sierra, we examine the economic effects of anti-theft protection spells on the economic decisions of beer retailers in the eastern Democratic Republic of the Congo. Such beliefs are present in all strata of society and have become more widely used with the rise in conflict in the region in recent decades. In this environment, retailers often experience theft, and expectations of theft lead retailers to lose business due to anxiety and frequent stock-outs. We extend access to retailers to undergo the rituals of three regional reputed witch doctors, and randomize the timing of the rituals. For retailers who report believing that the spell worked, the ritual reduces reported stress and the perceived risk of theft. Furthermore, as a result of the ritual, they purchased more bottles of beer, resulting in higher inventories and fewer stock-outs for weeks after the treatment, which increased sales and profits. None of these effects are found for retailers that did not believe in the efficacy of the ritual.

Part II

Research Papers

1 Shocks and Norm Conformity [1]

1.1 Introduction

Across societies, individuals want their behavior to match social norms, the shared behavioral standards of their communities. This desire for conformity alters people's publicly displayed behavior, even when they privately hold different preferences or beliefs. While a vast amount of experimental work, beginning with Asch's (1951) famous studies, show that norm conformity is an integral part of human psychology, recent work in cross-cultural psychology documents that norm conformity is more prevalent in some countries than in others (Gelfand et al., 2011). This difference confers an important trade-off to societies. On the one hand, by restricting the scope of acceptable behaviors, norm conformity promotes homogeneity and social cohesion, which facilitates collective action and reaching agreements in public policy; on the other hand, a lesser emphasis on norm conformity is conducive to diversity in behavior, attitudes, and beliefs, which may benefit creative innovation and long-run economic growth. Given the evidence, the question arises: Why is conformity to social norms in some societies more prevalent than in others? In particular, what leads some individuals to place greater importance in norm conformity than others?

According to scholars in psychology, anthropology, and evolutionary biology, the answer is that the extent to which societies assign importance to norm conformity has evolved as a function of external threat (e.g., Roos et al., 2015). The more individuals have been exposed to negative shocks that threaten their society's survival, the more they emphasize norm conformity today. To see how shocks can be an important determinant of norm conformity, consider a population confronting a series of social dilemmas, where cooperation produces the socially efficient outcome, but defecting is the individually rational strategy. Social norms often prescribe cooperation and punishment of defectors in such contexts. If a shock hits the population, then conformity to social norms becomes particularly beneficial because it promotes collective action and, therefore, increases the chances of survival. For example, societies that highly depend on agriculture for their survival live well when crops are plenty but suffer when there is a lasting drought. These societies increase their chances of survival if everyone follows the rules around crop cultivation and water consumption in times of need. More generally, this logic suggests that the more societies are exposed to negative collective shocks, the more beneficial it is to conform to the existing social norms.

I begin my analysis by providing a simple model that illustrates this logic. In it, individuals enter a prisoner's dilemma whose payoffs are determined by the economic condition in each period. Specifically, if a negative shock occurs, it uniformly reduces everyone's material payoff, possibly to subsistence consumption levels, in that period. Individuals may choose to invest in norm conformity, which promotes cooperation by imposing a cost on others who defect. The

[1] This chapter should be cited as Winkler, Max (2020): "Shocks and Norm Conformity", Mimeo.

model predicts that it is relatively more attractive for individuals to value norm conformity, the more the shock reduces their payoff toward the subsistence minimum. Hence, the logic suggests that a negative collective shock causes a society to place greater importance in norm conformity.

I take this hypothesis to the data and test for a relationship between individuals' experience of a variety of short-run negative shocks and the extent to which they value norm conformity. To measure the importance of norm conformity, I use a question that is included in two waves of the *World Values Surveys* (WVS). The WVS is a collection of nationally representative surveys on people's attitudes and values, covering roughly 100 countries, and also containing information on demographic characteristics, including year of birth and country of current residence.

To examine whether short-run shocks affect the importance people assign to norm conformity, I compile data on the occurrences of several forms of shocks, including three different types of natural disasters and major economic downturns. First, I use drought data from the CRU TS v.4.01 data set, which contains gridded measures of the self-calibrating Palmer Drought Severity Index (scPDSI). The data set is constructed from weather stations around the world, has a very find geographic resolution (Schrier et al., 2013). Second, I employ earthquake data come from the National Oceanic and Atmospheric Administration (NOAA), which provides geocoded information about earthquakes that were significant enough to create moderate damage or cause the loss of life. Third, I draw on hurricane data compiled by Yang (2008) and Mahajan and Yang (2020), who have constructed a "hurricane index" based on meteorological data on storm paths and intensities. Fourth, Barro and Ursúa (2008) assembled a data set of economic disasters in the form of large drops in national consumption.

I use an empirical strategy that is to estimate the relationship between individuals' lifetime experience of shocks and their emphasis on norm conformity. The lifetime experience measures account for experiences accumulated during an individuals' prior lifetime and allow for experience effects to decay over time, e.g., as memory fades or structural change renders early experiences less relevant. To construct these measures, I merge the occurrences of the four different types of shocks to the survey data based on respondents' birth year and country of residence. The empirical strategy exploits variation in exposure to these shocks over people's lifetime. I estimate a difference-in-differences model to examine the effect of shock experiences on the importance people place in norm conformity. Identification comes from cross-sectional differences in views on the importance of norm conformity and shock histories and from changes of those cross-sectional differences over time. All estimations include fixed effects that control for country of current residence $\times$ survey year, age, and birth year. Country of current residence $\times$ survey year fixed effects remove country-specific determinants of norm conformity and allow these determinants to change over the years of the survey data collection. Age fixed effects remove life cycle effects, such as age-related increases in importance of norm conformity. Birth year fixed effects remove effects specific to cohorts. By using within-country variation across cohorts, I exploit occurrences of shocks that are arguable uncorrelated with other determinants of the value placed on norm conformity. Any estimated difference in the importance of norm conformity should, therefore, capture the causal effect of shock-induced negative shocks.

I find positive and statistically significant relationships between the experience of negative shocks and the importance placed in norm conformity. Individuals who have lived through more

severe drought conditions, significant earthquakes, hurricanes, or economic downturns put a stronger emphasis on norm conformity. The estimates indicate that a one standard deviation increase in the shock experience is associated with roughly 4-9% of a standard deviation increase in the importance of norm conformity. These effect sizes are substantial. They correspond to ca. 3-8% of the gap between the average emphasis of norm conformity in the Netherlands and Egypt, which are among the countries with the lowest and highest emphasis on norm conformity in the data.

I perform several robustness checks to assess the sensitivity of the results. First, I add controls for individuals' educational attainment and employment status. All results go through if I account for these additional (more endogenous) covariates. Second, I rerun the regressions restricting the sample to native-born individuals. In the main analysis, I use the country where the survey was conducted to determine the respondents' residences during their whole lifetime. This procedure raises two concerns: (*i*) selective migration could drive the result if, for example, individuals who place little importance in norm conformity migrate from countries with frequent shocks to countries with fewer shocks; and (*ii*) the method may introduce measurement error among those individuals who recently immigrated to their current country of residence. To address this concern, I restrict the sample to native-born individuals. Excluding immigrants from the sample leads to qualitatively similar results, although a substantially smaller sample size implies that not all coefficients are statistically significant at conventional levels. Third, I test the influence of single countries and cohorts on the estimates. I rerun the main specifications several times, each time excluding one country or one cohort. Again, I obtain very similar estimates.

In the final exercise, I extend the logic of the model by examining the relationship between positive economic conditions and the importance of norm conformity. I compute individuals' lifetime experience of consumption growth rates and test whether individuals who have experienced higher growth rates in their prior lifetime place less importance in norm conformity today. I find a negative and statistically significant relationship. The estimates indicate that a one standard deviation increase in consumption growth experience is associated with roughly 5% of a standard deviation decrease in the importance of norm conformity. As before, the effect size is substantial. It corresponds to ca. 4-5% of the gap between the Netherlands and Egypt.

In sum, the empirical analyses yield a coherent conclusion. Be it in the form of natural disasters or major economic downturns, the experience of negative shocks increases the importance people place in conformity to the existing social norms. Conversely, people who have lived through periods of high economic growth put less emphasis on norm conformity.

My findings contribute to several strands of the literature. First, they add to a growing body of work that empirically investigates the determinants of cultural traits and norms (Nunn and Wantchekon, 2011; Alesina et al., 2013; Guiso et al., 2016; Becker et al., 2016; Buggle and Durante, 2017; Lowes et al., 2017; Becker, 2019; Enke, 2019; Schulz et al., 2019; Giuliano and Nunn, 2020). The literature has established that today's differences in norms and cultural traits are determined through an evolutionary process, and that they matter for economic development. However, these differences are small when compared to variability in cultural traits *within* societies (Desmet et al., 2017). By studying differences in norm conformity, a "meta trait" that

abstracts from any particular norm or cultural trait but regulates the heterogeneity of culture in society, this paper provides a rationale for why some societies are more culturally diverse than others.

Second, my paper ties into recent work that studies the rise in cultural polarization in recent decades (Bertrand and Kamenica, 2018; Desmet and Wacziarg, 2020). Since norm conformity entails cultural homogeneity within groups, it may result in a cultural divide between groups if they conform to different norms. While these studies treat the extent of norm conformity as an exogenous parameter, my paper provides an explanation for why societies become culturally more polarized. This also relates to a broader literature, originating in political science and sociology, that examines cultural change arising from modernization and economic prosperity (Inglehart, 1997; Inglehart and Baker, 2000) and cultural backlash and political polarization as a consequence of economic insecurity and inequality (Autor et al., 2016; Inglehart, 2018; Fetzer, 2019; Bo et al., 2019).

Third, a growing amount of experimental work suggests that short-run shocks can lead to cultural change. These papers have mainly focused on conflict between groups. They show that individuals who were exposed to conflict behave more cooperatively toward their group members in behavioral experiments (Bellows and Miguel, 2009; Gneezy and Fessler, 2012; Voors et al., 2012; Cassar et al., 2013; Bauer et al., 2014; Gilligan et al., 2014; Bauer et al., 2016; Lowes and Montero, 2018), and that religiosity may be a proximate mechanism (Henrich et al., 2019). This paper suggests another proximate mechanism: stronger conformity to prosocial norms.

The remainder of the paper is organized as follows. The next section lays out a stylized model of the evolution of norm conformity as a function of collective negative shocks. The model shows how a strong emphasis on norm conformity is a cultural adaption to a history of negative shocks. Section 1.3 describes the data used in the analysis and the measurement of lifetime shock experiences. Section 1.4 presents the strategy of the empirical analysis and reports the results. Section 1.5 concludes.

1.2 Conceptual Framework

This section lays out a conceptual framework for how negative collective shocks may affect the importance of norm conformity in society. I present a purposefully simple and stylized model to structure and motivate the empirical analysis. The insight that emerges from the model is that it is relatively more beneficial to value norm conformity (and comply with the norms) in response to negative shocks. Intuitively, this is because norm conformity facilitates collective action, enabling any society to operate more efficiently. These efficiency gains are particularly valuable when coping with the consequences of negative shocks. This mechanism is akin to several models on the origins of large-scale cooperation and its evolution (e.g. Andrés Guzmán et al., 2007; Boyd et al., 2011; Roos et al., 2015).

Setup. The players of the game consist of a continuum of members of a society. Time is discrete, and players live forever. In each period, players enter a prisoner's dilemma with a random other player, where they choose to either cooperate (C) or defect (D), $a_i \in \{C, D\}$. The material payoffs in each period are summarized in Table 1.1. Here, $t > r > p > s$ are standard

prisoner's dilemma parameters. In addition, material payoffs include a loss which depends on the realization of the shock random variable, $\Delta_t \in [0, \delta]$, where $\delta < 1$. If a shock occurs, it uniformly reduces material payoffs by a fraction of δ. For example, a drought destroys half of the crops, independent of the actions individuals adopt. Finally, each player needs a minimum payoff to survive, $\mu \in (0, s - \delta s)$, which is positive, but smaller than the lowest possible material payoff to avoid negative payoffs. The subsistence payoff is subtracted from all payoffs. Denote player i's net material payoffs from the prisoner's dilemma by y_i.

Table 1.1: Prisoner's Dilemma

	C(ooperate) / T(ight)	D(efect) / L(oose)
C(ooperate) / T(ight)	$r - \Delta_t r - \mu$, $r - \Delta_t r - \mu$	$s - \Delta_t s - \mu$, $t - \Delta_t t - \mu$
D(efect) / L(oose)	$t - \Delta_t t - \mu$, $s - \Delta_t s - \mu$	$p - \Delta_t p - \mu$, $p - \Delta_t p - \mu$

Notes: $t > r > p > s$ are standard prisoner's dilemma parameters. $\Delta_t \in [0, \delta]$ is the shock random variable, where $\delta < 1$. It is positive in period t with some small probability. If a shock occurs, it uniformly destroys material payoffs by a fraction of δ. $\mu \in (0, s - \delta s)$ is the minimum level of consumption players need to survive.

Apart from caring about their material payoff, players potentially bear a cost if they deviate from the social norms. I assume that cooperating is the social norm in the prisoner's dilemma. Hence, players may face a cost if they choose to defect. This cost depends on γ, the severity of social disapproval or punishment of norm violators.

At several points throughout their lives, players choose one of two possible strategies. The first strategy values norm conformity and places strong importance on avoiding behaviors that others consider wrong. I call players who choose this strategy "tight (T)". Tight players never deviate from the social norm and, therefore, always cooperate in the prisoner's dilemma. However, there is a cost to adopting and maintaining the tight strategy in each period. This cost arises because tight players, to avoid behaviors that others consider wrong, cannot always choose the behaviors that they prefer the most. The cost is proportional to players' material payoffs, κy_i, where $\kappa > 0$.[2] The second strategy does not value norm conformity and places no importance in avoiding behaviors that others consider wrong. I call players who choose that strategy "loose (L)". Loose players always defect in the prisoner's dilemma.

Players' strategy choices are persistent. While players interact with a randomly chosen other player each period, they can only occasionally revise their strategy. More precisely, each player maintains her strategy for T periods, where T is an independent draw from an exponential distribution with rate R. When a player has the opportunity to revise her strategy, she follows a simple rule: she compares her current payoff with those from a random sample of other members in society and, with probability proportional to the difference in payoffs, switches her strategy

[2]More precisely, $\kappa \in (0, (r + \gamma - t)/(r - \mu))$ where $(r + \gamma - t)/(r - \mu) < 1$. If $\kappa > (r + \gamma - t)/(r - \mu)$, then the cost is prohibitively high and no one will adopt and maintain the tight strategy. I focus on the empirically relevant case where both strategies are present in the population. The condition also implies upper and lower bounds for γ, i.e., $t - r + \kappa(r - \mu) < \gamma < t - \mu$. If γ did not meet this condition, then again only one of two strategies were present in the population.

if the other strategy yields a higher payoff.

In each period, a shock may or may not hit society. If it occurs, a shock affects the relative attractiveness of the two strategies by increasing the relative benefit of cooperation in the prisoner's dilemma. Since players can revise their strategies only occasionally, the effect of shocks on the distribution of strategies adopted in society will persist over several periods.

Expected payoffs. I now turn to an examination of the expected payoffs of both strategies. Let π_t^s denote the expected payoff from choosing the tight ($s = T$) or the loose ($s = L$) strategy in period t. Given the random matching process, the average payoffs are a function of the players interacting with each other and the share of tight players in the population. I let $\alpha \in [0, 1]$ denote this share and interpret α as the measure of the overall importance of norm conformity in the society. I first consider the expected payoff of the tight strategy. Tight players receive the expected material payoff from the prisoner's dilemma and bear the cost of valuing norm conformity and avoiding behaviors that others consider wrong. In the prisoner's dilemma, they benefit from a cooperative interaction and receive r if matched with another tight player. However, they risk receiving the sucker's payoff s if matched with a loose player. If a shock hits, it reduces all material payoffs by a fraction of Δ_t. The subsistence payoff μ is subtracted, and the cost of valuing norm conformity κ is a fraction of the remaining material payoff. Thus, the expected payoff for the tight strategy is given by:

$$\text{Tight strategy (norm adherers):} \quad \pi_t^T = \underbrace{(1-\kappa)}_{\substack{\text{net of cost of valuing} \\ \text{norm conformity}}} \Big[\underbrace{(1-\Delta_t)}_{\text{shock}} \big[\underbrace{\alpha r + (1-\alpha)s}_{\substack{\text{expected PD payoff} \\ =y^T}} \big] \underbrace{-\mu}_{\substack{\text{subsistence} \\ \text{payoff}}} \Big] \tag{1.1}$$

Loose players, receive the expected material payoff from the prisoner's dilemma, and face the punishment cost for deviating from the social norm γ if they defect in an interaction with a tight player. In the prisoner's dilemma, they gain the temptation payoff t if matched with a tight player. They receive p if matched with another loose player. Again, a shock cuts material payoffs by a fraction of Δ_t – including the punishment cos γ – and the subsistence payoff μ is subtracted. Thus, the expected payoff for the loose strategy is given by:

$$\text{Loose strategy:} \quad \pi_t^L = \underbrace{(1-\Delta_t)}_{\text{shock}} \big[\underbrace{\alpha t + (1-\alpha)p}_{\substack{\text{expected PD payoff} \\ =y^L}} \underbrace{-\alpha\gamma}_{\substack{\text{expected cost of} \\ \text{norm violation}}} \big] \underbrace{-\mu}_{\substack{\text{subsistence} \\ \text{payoff}}} \tag{1.2}$$

Shocks and importance of norm conformity. The evolution of the importance placed in norm conformity is captured by the growth rate of adoption of the tight strategy, which equals the standard replicator dynamic (Sandholm, 2010). Intuitively, the strategy with the higher expected payoff propagates faster, and the dominated strategy is progressively eliminated. Let π denote the average payoff across both strategies, $\pi = \alpha\pi^T + (1-\alpha)\pi^L$, then the growth rate

is given by

$$\begin{aligned}
\dot{g}^T = \frac{d\alpha}{dt} &= \alpha(\pi^T - \pi) \\
&= \alpha(\pi^T - (\alpha\pi^T + (1-\alpha)\pi^L)) \\
&= \alpha(1-\alpha)(\pi^T - \pi^L) \\
&= \alpha(1-\alpha)\Big[\underbrace{(1-\Delta_t)\alpha\gamma}_{\substack{\text{relative gain from} \\ \text{avoiding cost of} \\ \text{norm violation}}} - \underbrace{(1-\Delta_t)(y^L - y^T)}_{\substack{\text{relative loss from not} \\ \text{defecting in the PD}}} - \underbrace{\kappa((1-\Delta_t)y^T - \mu)}_{\substack{\text{cost of valuing} \\ \text{norm conformity}}} \Big] \\
&= \beta_0 + \beta_1\Delta_t
\end{aligned} \tag{1.3}$$

where $\beta_0 = \alpha(1-\alpha)(\alpha\gamma + \kappa\mu - \alpha(t+\kappa r - r) - (1-\alpha)(p+\kappa s - s)) < 0$, and $\beta_1 = \alpha(1-\alpha)(\alpha(t+\kappa r - r) + (1-\alpha)(p+\kappa s - s) - \alpha\gamma) > 0$.

Thus, a negative shock has a positive effect on the growth rate of the importance placed in norm conformity in society. Since players can revise their strategies only occasionally, the effects of shocks persist for several periods. This generates the following hypothesis.

Hypothesis 1. *The more individuals have experienced negative collective shocks in their prior life, the more they place importance in conformity to social norms today.*

In sum, the model presented here shows how negative collective shocks Δ_t result in greater importance placed in norm conformity α. I now turn to the data description and empirical analysis, which tests the hypothesis.

1.3 Data and Measurement

My difference-in-differences empirical strategy compares the importance placed on norm conformity of individuals who experienced different degrees of collective negative shocks across countries. This empirical strategy requires measurement of individuals' emphasis on norm conformity and their shock experience.

1.3.1 Dependent Variable: Importance of Norm Conformity

To measure the importance people place on norm conformity, I use a question from the *World Values Surveys* (WVS), a data set that consists of a series of nationally representative surveys covering roughly 100 countries from around the world. It contains information on different types of attitudes, values, and preferences, as well as information on standard demographic characteristics, such as sex, age, education, and labor market status. In two survey waves during the years 2006-2009 and 2010-2014, respondents are given the description of a person: It is important to this person to always behave properly; to avoid doing anything people would say is wrong. Respondents then choose the answer that best describes how similar this person is to them: very much like me; like me; somewhat like me; a little like me; not like me; and not at all like me. I code the responses to create a variable with integer values from 16, increasing with the value placed on proper behavior (or norm conformity).

1.3.2 Independent Variables: Lifetime Shock Experiences

Individuals may experience many different types of negative shocks. Among the most important sources of negative shocks in humans' history were wars, natural disasters, diseases, and famines. To test the hypothesis that collective negative shocks increase the importance of norm conformity, I collect data on natural disasters and major economic downturns. I focus on these shocks for several reasons. First, they are easy to measure and comparable across countries and over time. Among the various types of natural disasters, droughts, earthquakes, and hurricanes (or tropical storms more generally) were most devastating in recent decades (Ritchie, 2014). Second, different natural disasters typically affect different parts of the world. For example, certain countries frequently suffer from earthquakes but never see a hurricane, and vice versa. Third, the strategy has the key advantage that it reduces the likelihood that unobserved characteristics specific to a particular shock drive the results or that any individual result is a false positive. Fourth, natural disasters have the appealing feature that they are clearly exogenous.

Droughts. The drought data come in the form of the self-calibrating Palmer Drought Severity Index (scPDSI) provided by the Climatic Research Unit.[3] The CRU TS v.4.01 data set contains gridded measures for the scPDSI at monthly frequency for the whole world at a 0.5-degree resolution (approx. 55km). The data set is constructed from weather stations around the world and available from 1901 (Schrier et al., 2013).

I use geospatial software to aggregate the gridded drought data to the country-year level. Following standard procedures (e.g., Dell et al., 2012), I compute population-weighted average drought conditions for each country and year, where the weights are constructed from 2000 population data at 0.5-degrees resolution from the Global rural-urban Mapping project (Balk et al., 2006). I multiply the scPDSI values by minus so that higher numbers indicate more severe drought conditions. The resulting data set contains yearly average drought intensity measures for countries covered by the WVS since 1902. Appendix Figure A.1 shows the data for the countries included in the analysis. According to the PDSI definition, values greater than 2 indicate moderate drought conditions on average in a given country and year.

Earthquakes. The earthquake data come from the National Oceanic and Atmospheric Administration (NOAA). It provides a geocoded data set that records information about each earthquake if it was significant enough to create moderate damage or cause the loss of life. An earthquake is classified as significant in the database if it meets one of the following criteria: Moderate damage (approximately $1 million or more), ten or more deaths, Magnitude 7.5 or greater, Modified Mercalli Intensity Xs or greater, or the earthquake generated a tsunami.

To construct earthquake exposure measures at the country-year level, I proceed as follows. Starting with the 2000 population gridded data at 0.5-degrees resolution from the Global rural-urban Mapping project, I assign earthquakes with Richter magnitude of 6 (strong) or greater to grids if they occurred within a radius of 100km to the grids' centroids.[4] For each grid and year,

[3] See Palmer (1965) for the origin of the drought severity index and details on its construction.

[4] These thresholds have been used in the literature before, for example, by Bentzen (2019).

I construct an earthquake intensity index by computing

$$x_{igt} = \frac{(eq_{igt} - 6)^2}{(eq^{max} - 6)^2} \tag{1.4}$$

where eq_{igt} is the Richter magnitude of earthquake i assigned to grid g in year t. eq^{max} is the maximum magnitude observed in the data, 9.5 on the Richter scale. A grid's earthquake exposure in a given year is then the sum of all x_{igt} in that year, and a country's exposure is the population-weighted average of all grid-level values. The resulting data set contains yearly average earthquake intensity measures for countries included in the WVS since 1850. Appendix Figure A.2 displays these data.

Hurricanes. The hurricane data come from Yang (2008) and Mahajan and Yang (2020), who construct a "hurricane index" based on meteorological data on storm paths and intensities.[5] The data set provides estimates of the average storm exposure of residents in a given country and year since 1950. The construction of the index is very similar to the procedure described for my earthquake exposure measure. The index is highly predictive of disaster damages and human losses experienced by countries in particular years. Appendix Figure A.3 exhibits these the index for countries included in the analysis over time.

Economic disasters. Barro and Ursúa (2008) have compiled time-series data on consumption from 1870 to today for a sample of 42 countries. The authors define periods of economic disasters when consumption drops by 10% or more from peak to trough. I rely on this definition and set an indicator equal to one in years when a country goes through a consumption disaster. Appendix Figure A.4 shows the evolution of this indicator for countries included in the analysis.

Defining lifetime shock experiences. To test whether lifetime shock experiences affect the importance of norm conformity, I construct measures of past experiences that apply the weighting function estimated in prior work in the context of unemployment experience (Malmendier and Nagel, 2011; Malmendier and Shen, 2019). Specifically, I measure the shock experience accumulated by time t as

$$Shock\ Experience_t = \sum_{k=0}^{t-1} w(t,k)\ Shock_{t-k} \tag{1.5}$$

where $Shock_{t-k}$ is the shock realization in year $t-k$, and k denotes the time lag. Weights w are a function of t and k,

$$w(t,k) = \frac{t-k}{\sum_{k=0}^{t-1}(t-k)} \tag{1.6}$$

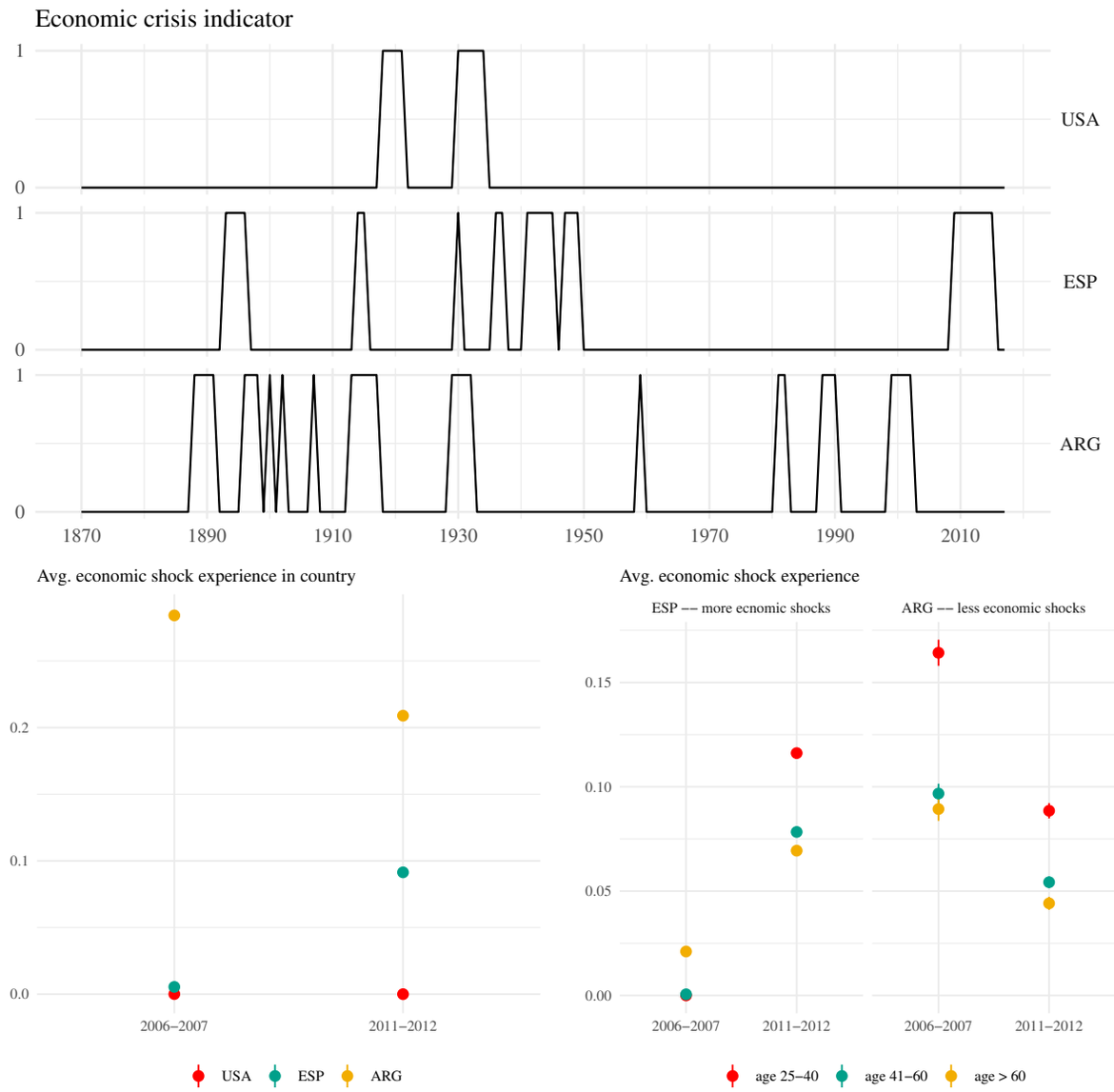
This weighting specification accounts for all experiences accumulated during an individuals lifetime. It allows for experience effects to decay over time, e.g., as memory fades or structural

[5]The term "hurricane" is typically used to describe severe tropical storms in the Atlantic and eastern Pacific. However, the same type of event is known as a "typhoon" in the western Pacific and "tropical cyclone" in the Indian Ocean and Oceania. A tropical storm is classed as a hurricane if sustains winds above 74 miles (119 kilometers) per hour.

change renders old experiences less relevant. Thus, the weighting scheme emphasizes recent experiences, letting them carry higher weights, while still allowing for an impact of earlier life histories.

Appendix Figures A.5 to A.8 display the average experiences of droughts, earthquakes, hurricanes, and economic disasters across countries.

Figure 1.1: Economic shocks and average economic shock experience in selected countries



Notes: The figures illustrate how shock experiences map into the independent variables of the analysis. The top panel shows the economic shock indicator time series for the U.S., Spain, and Argentina; the bottom-left panel plots the average experiences for each of the three countries; and the bottom-right panel shows the mean economic shock experiences for young (age below 40), middle-aged (between 41 and 60), and old individuals (above 60) in Spain (left plot) and Argentina (right plot). Economic shock experiences are computed as described in equations 1.5 and 1.6. The plots highlight the three sources of variation that are central to the identification strategy. Differences in individuals' lifetime experiences depend on their cohort and country of residence, and these evolve. The younger the individuals, the more important are the most recent shock experiences.

Illustrating how shocks shape shock experience. To illustrate how shock occurrences translate into the main independent variables in the analysis, the top panel in Figure 1.1 displays the economic shock indicator of Barro and Ursúa (2008) for the United States, Spain, and Argentina. The United States experienced a contraction larger than 10% in national consumption after World War I and during the Great Depression in the 1930s; Spain saw such downturns frequently during the first half of the 20th century and, most recently, after the financial crisis of 2008; Argentina experienced large economic shocks throughout the century, most recently around 2000. The bottom panels in Figure 1.1 illustrate how these shock time series translate into individuals' lifetime shock experiences, both in the cross-section and over time. The bottom-left panel plots the average experiences for each of the three countries, and the bottom-right panel plots the average experiences for young (age below 40), middle-aged (between 41 and 60), and old individuals (above 60). The plots highlight the three sources of variation that are central to the identification strategy. Differences in individuals' lifetime experiences depend on their cohort and country of residence, and these evolve. First, in the bottom-left panel, the average economic shock experiences among Americans is close to zero in years 2006-2007 (first survey wave) because the Great Depression occurred more than 70 years ago, and it remains close to zero in years 2011-2012 (second survey wave) because no major economic shock hit the U.S..[6] Similarly, the average economic shock experience was close to zero in Spain in years 2006-2007. However, Spain experienced a massive economic downturn in 2008, so that its average economic shock experience increases in the years 2011-2012. By contrast, the average economic shock experience in Argentina is high in the years 2006-2007, and it slightly decreases by 2011-2012 because no economic shock occurred in-between. Second, the bottom-right panel illustrates how shocks affect the economic shock experience of different age groups. Younger cohorts' shock experiences react the most to recent events. In Spain, for example, the economic shock experience increases more strongly among the young than among the old; similarly, the economic shock experience decreases the most among the young and the least among the old in Argentina. As defined in equations 1.5 and 1.6, the younger the individuals, the more important are the most recent shock experiences.

Table 1.2 shows the summary statistics. I focus on individuals from age 25 to 75. The data set contains 126,808 observations from 78 countries. The mean importance that people assign to norm conformity is 4.44.

[6]The drop in consumption during the Great Recession following the financial crisis of 2008 was smaller than 10%.

Table 1.2: Summary statistics main analysis

Statistic	N	Mean	St. Dev.	Min	Max
Dependent variable					
Importance of norm conformity	126,808	4.440	1.361	1	6
Negative shocks					
Drought intensity experience	123,962	0.367	0.652	−1.643	2.242
Drought × High share agriculture	123,962	0.222	0.378	−0.591	1.905
Earthquake experience	126,808	0.007	0.017	0	0.131
Hurricane experience	85,195	0.002	0.009	0	0.075
Economic disaster experience	67,118	0.046	0.073	0	0.373
Individual covariates					
Birth year	126,808	1964.5	14.0	1931	1997
Age	126,808	44.8	13.5	25	75
Gender, male	126,808	0.476	0.499	0	1
Unemployed	124,002	0.087	0.282	0	1
Education	116,616	5.782	2.239	2	9

Notes: Importance of norm conformity is taken from waves 5 and 6 of the World Values Survey (WVS) conducted in years 2006-2009 and 2010-2014. *Drought intensity experience* measures individuals' lifetime experience of PDSI drought intensity conditions. The drought data comes from the Climatic Research Unit's TS v.4.01. I construct country-year level values from these data, as described in the text. *Drought × High share agriculture* denotes the interaction of the lifetime experience of PDSI drought intensity conditions with an indicator equal to one if the average country share of employment in agriculture in years 2000 to 2005 is above 25%. The information on the employment share in agriculture comes from the World Bank's World Development Indicators (WDI). *Earthquake experience* represents individuals' lifetime experience of significant earthquakes. The data come from the NOAA. *Hurricane experience* denotes individuals' lifetime experience of hurricanes and other tropical storms. The data come from Yang (2008) and Mahajan and Yang (2020). *Economic disaster experience* is individuals' lifetime experience of large drops in national consumption. The data come from Barro and Ursúa (2008). I construct all experience variables as weighted lifetime averages following the procedure in Malmendier and Nagel (2011) and Malmendier and Shen (2019). The WVS also contains individual-level covariates, including respondents' birth year, age, gender, an unemployment status, and highest educational attainment.

1.4 Results

I now turn to the estimates of the relationship between people's experiences of negative shocks and the importance of norm conformity today. In this section, I describe the empirical strategy and present the results.

1.4.1 Empirical Strategy

I exploit variation in exposure to negative shocks over people's lifetime. Combining data from various sources discussed in Section 1.3, I estimate a difference-in-differences model to examine the effect of shock experiences on the importance people place in norm conformity. My baseline specification is the following:

$$y_{ibct} = \beta \; Shock \; Experience_{bc} + \sum_{ct} \alpha_{ct} + \sum_{b} \delta_b + \gamma' x_i + \varepsilon_{ibct} \tag{1.7}$$

where y_{ibct} is the importance placed in norm conformity by individual i from birth cohort b and resident in country c; and α_{ct} and δ_b are full sets of country of current residence $\times$ survey year and birth cohort fixed effects. $Shock \; Experience_{bc}$ refers to the four different shock experience measures which vary at the birth-cohort $\times$ country level: (i) drought experience; (ii) earthquake experience; (iii) hurricane experience; and (iv) and economic downturn experience. I analyze the shocks in four separate regressions, each regression examining the effect of one of the shock experiences on the dependent variable. I assign the shock experience measures to individuals based on the country they reside in at the time of the survey. In the baseline specification, x_i denote a gender indicator equal to one if i is male and a full set of age fixed effects. I allow that idiosyncratic differences, ε_{ibct}, are correlated across individuals within a given birth-cohort $\times$ country.

β is the main coefficient of interest, capturing the effect of collective negative shock experiences during individuals' prior lifetime. By conditioning on country of current residence $\times$ survey year fixed effects, the empirical specification absorbs country-specific determinants of norm conformity such as institutions, deep history, or current economic conditions and allow these determinants to change over time of the survey data collection. By conditioning on birth cohort fixed effects, the specification also absorbs all variations across cohorts that might induce different degrees of norm conformity. Finally, by including age fixed effects, the specification removes life cycle effects such as age-related increases in the value place in norm conformity.

In addition to the baseline specification, I estimate specifications that: (i) use alternative clustering choices (country level, cohort level, and country-cohort two-way clustering); and (ii) include additional individual-level (an indicator equal to one if i is unemployed and fixed effects for highest educational achievement). Note that these individual-level controls may be "bad controls" because negative shocks may also affect individuals' employment status and educational choices. Still, including these controls in separate regressions allows me to examine the robustness of the results.

1.4.2 Identifying Assumption

The identification of shock experience effects comes from (*i*) within-country differences across cohorts and (*ii*) cross-country differences in the importance of norm conformity and shock histories and how they evolve. For example, the first comparison is between a U.S. resident born in 1958 to a U.S. resident born in 1975; the second comparison is between a U.S. resident born in 1958 and an individual born in the same year but resident in Japan. The effect is identified if $E(\varepsilon_{it}|\alpha_{ct}, \delta_b, x_i) = 0$ is satisfied. That is, the identification assumption is violated if unobserved factors at the country-cohort level are systematically correlated with both the shock occurrences and the importance placed on norm conformity. Natural disasters are arguably exogenous to individuals. Fixed effects for countries of residence and birth cohorts remove the most obvious confounding factors of major economic downturns.

1.4.3 Baseline Results

Figure 1.2 presents an overview of the main results from the baseline difference-in-differences specification in equation 1.7. The bars depict the estimated effects of different types of natural disaster experiences and major economic downturns experience on the importance individuals place in norm conformity today. Each bar corresponds to a distinct natural disaster (droughts, significant earthquakes, or hurricanes) or economic downturns in the form of large drops in national consumption.[7]

Several consistent patterns emerge. First, all shock experiences are associated with an increase in the importance placed in norm conformity. All estimates are positive and statistically significant. Second, the point estimates are very similar across shocks. It is largest for hurricanes and smallest for economic disaster, although the differences lie within the confidence intervals. Note that all specifications include country of current residence fixed effects. This implies that the effect is not merely driven by differential exposure to natural and economic disasters across countries, but by differential experiences across cohorts within countries and how these differences evolve between countries.

Table 1.3 reports the details of the estimation. Based on data from 76 countries from around the world, drought experience generally leads to a higher importance of norm conformity, but the effect is not statistically significant at conventional levels (Column 1). The effect doubles in size and becomes highly statistically significant when drought experience is interacted with an indicator equal to one if the country share of employment in agriculture is above 25% (the median share in the sample). This finding is consistent with the interpretation that drought shocks have particularly adverse effects in countries where agriculture is a dominant share of economic activity. In column 3, I additionally control for educational attainment and an indicator equal to one if the respondent is unemployed. The result goes through if I account for these additional (more endogenous) covariates, although the effect becomes smaller and is estimated with less precision.

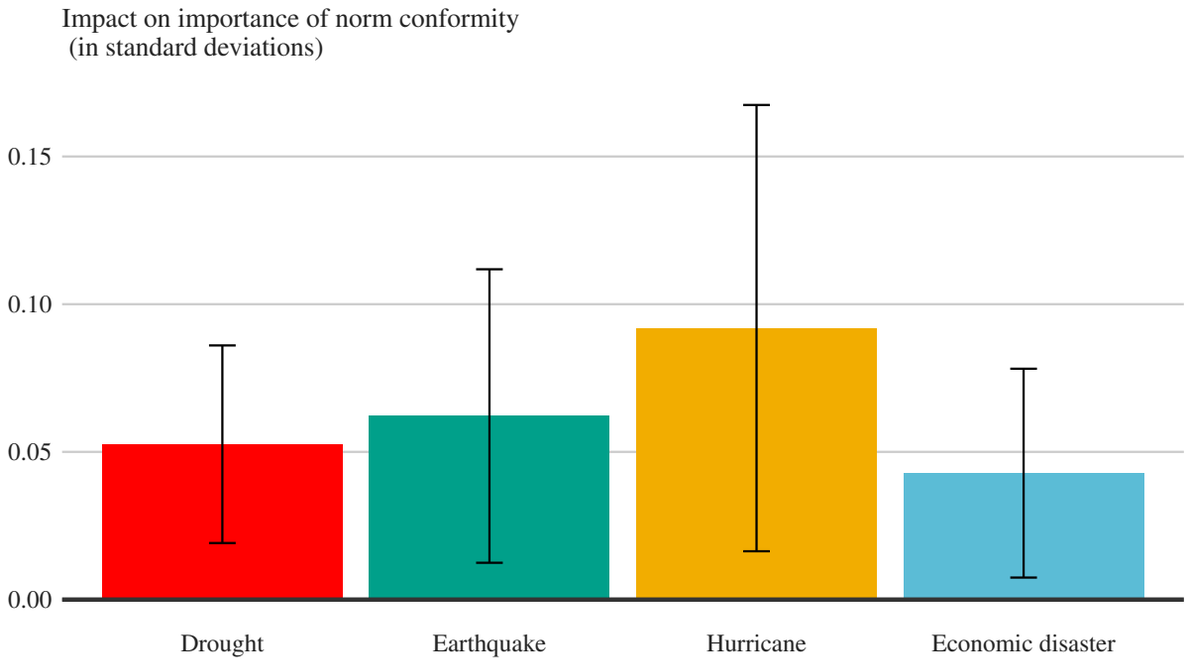
[7]The bar on drought shows the estimated coefficient of the interaction of individuals' drought experience × an indicator equal to one if the national share of employment in agriculture is higher than 25%. This interaction captures the logic that drought shocks have severe effects when a country depends on agriculture.

Table 1.3: The effect of natural and economic disaster experience on importance of norm conformity

	Importance of norm conformity [std.]								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Drought experience [std.]	0.025	−0.015	−0.019						
	(0.015)	(0.020)	(0.021)						
Drought experience × High agriculture [std.]		0.053***	0.036*						
		(0.017)	(0.019)						
Earthquake experience [std.]				0.062**	0.070**				
				(0.025)	(0.027)				
Hurricane experience [std.]						0.092**	0.100**		
						(0.039)	(0.039)		
Economic disaster experience [std.]								0.043**	0.047***
								(0.018)	(0.018)
No. of countries	76	76	76	78	78	66	66	33	33
Country × survey-year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Cohort, age, gender FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Other individual controls	No	No	Yes	No	Yes	No	Yes	No	Yes
N	123,962	123,962	111,424	126,811	114,185	85,195	77,972	67,111	61,541
R^2	0.125	0.125	0.126	0.123	0.124	0.129	0.128	0.122	0.123

Notes: OLS estimates, robust standard errors are clustered at the country-cohort level and reported in parentheses. An observation is an individual. The outcome in all columns is self-reported importance of norm conformity. The main independent variables are lifetime experience of drought intensity interacted with an indicator equal to one if the average share of employment in agriculture in years 2000 - 2005 is greater than 25%, lifetime experience of significant earthquakes, lifetime experience of hurricanes, and lifetime experience of economic disasters. The data sources of the natural and economic disasters and the measurement of lifetime experience are described in the text. All regressions include fixed effects for country of current residence × survey year, birth cohort, age, and gender. Other individual controls include a dummy equal to one if the respondent is unemployed and fixed effects for highest educational attainment. All variables are standardized to z-scores. *** $p < 0.01$, ** $p < 0.05$, and * $p < 0.1$.

Figure 1.2: OLS estimates of the effects of negative shock experiences on importance of norm conformity



Notes: The figure reports the estimated coefficient from regressions of respondents' self-reported importance of norm conformity on their lifetime experiences of several types of natural disasters (droughts, earthquakes, hurricanes) and large economic downturns. All regressions include fixed effects for country of current residence × survey-year, birth cohort, age, and gender. The confidence intervals are at the 95% level. Standard errors are clustered at the country of residence × birth-cohort level. The dependent variable comes from the World Values Survey. The data sources of the natural and economic disasters and the measurement of lifetime experience are described in the text. All variables are standardized to z-scores. *Drought* reports the interaction of lifetime drought experience × an indicator equal to one if the average share of employment in agriculture in years 2000 - 2005 is greater than 25%, as described in the text.

Columns 4 to 9 present the estimation results for earthquakes, hurricanes, and economic disasters. The effects are positive and statistically significant across all specifications.

1.4.4 Effect Magnitudes

Are the statistically significant effects of natural disasters and economic downturns substantially important? I assess their magnitude in two ways. First, I compare the effect sizes to the distance between the average importance of norm conformity in the Netherlands and Egypt, which are among the countries that emphasize norm conformity the least (the Netherlands) and the most (Egypt). One standard deviation more hurricane experience is associated with 7.8% of the distance between the two countries, a substantial effect size. For earthquakes, the effect corresponds to 5.3% of the distance; for economic downturns, it is 3.7%; and for droughts in agricultural countries, it is 3.2%.

Second, I compare the magnitudes to the correlations between the importance of norm conformity and several other important factors. For example, individuals who have completed

secondary school or above tend to put less emphasis on norm conformity by 0.02 units.[8] The estimated coefficients in Table 1.3 are between twice (economic disaster) and 4.5 times (hurricanes) larger than this correlation. To give a second example: the effect sizes are between 0.2 and 0.45 times the correlation between the importance of norm conformity and an indicator if individuals are religious. These comparisons also suggest that the effect magnitudes are substantial.

1.4.5 Robustness Checks

I also perform two robustness checks. First, I rule out that measurement error or selective migration drive the estimated effect. Second, I assess the sensitivity of the results to individual countries and cohorts.

Selective migration does not drive the results. To construct individual-level estimates of shock experiences, I assign occurrences of natural disasters and economic downturns to individuals based on the country they reside in at the time of the survey. The procedure raises the concern that selective migration may drive the result. For example, a positive association between shocks and norm conformity would emerge if individuals who place little importance on norm conformity migrate from countries hit by shocks to countries not hit by shocks. To address this concern, I restrict the sample to native-born individuals. Compared to the main analysis, this robustness check loses statistical power because only one of the two WVS waves provides information on respondents' immigration status. Thus, the sample sizes drop by more than 50%. Appendix Table A.1 reports the results. The estimates are qualitatively similar, although the substantially smaller sample sizes imply that not all coefficients are statistically significant at conventional levels.

Individual countries or cohorts do not account for the results. I also assess the sensitivity of the results to the exclusion of individual countries and cohorts. I rerun the specifications but drop one country or cohort at a time. Figures A.9 to A.12 report the results. The estimated coefficients are very similar to the baseline results.

1.4.6 It the Effect Symmetric?

Does norm conformity become less prevalent in good times? I extend the logic of the model by examining the relationship between positive economic conditions and the importance of norm conformity. I compute individuals' experience of consumption growth rates using the consumption data of Barro and Ursúa (2008) and the exact same procedure as before. Then, I test whether people who have experienced higher growth rates in their prior lifetime place less importance in norm conformity today. Table 1.4 reports the results. I find a negative and statistically significant relationship. Column 1 indicates that a one standard deviation increase in consumption growth experience is associated with roughly 5% of a standard deviation increase in the importance of norm conformity. The effect size is very similar to the effects of negative shocks but points in the opposite direction. Column 2 shows that controlling for additional

[8] To be consistent, I compare the effect sizes from the estimation to the conditional correlation between norm conformity and educational attainment, net of country of residence × survey year fixed effects.

individual-level covariates increases effect to close to 7% of a standard deviation. Thus, the effect size corresponds to ca. 4-5% of the gap between the average emphasis of norm conformity in the Netherlands and Egypt.

Table 1.4: The effect of economic growth experience on importance of norm conformity

	Importance of norm conformity [std.]	
	(1)	(2)
Economic growth experience [std.]	-0.047^{**}	-0.067^{***}
	(0.022)	(0.022)
No. of countries	33	33
Country × survey-year FE	Yes	Yes
Cohort, age, gender FE	Yes	Yes
Other individual controls	No	Yes
N	66,955	61,441
R^2	0.122	0.124

Notes: OLS estimates, robust standard errors are clustered at the country-cohort level and reported in parentheses. An observation is an individual. The outcome in all columns is self-reported importance of norm conformity. The main independent variables are lifetime experience of growth rates in national consumption. The data sources and the measurement of lifetime experience are described in the text. All regressions include fixed effects for country of current residence × survey year, birth cohort, age, and gender. Other individual controls include a dummy equal to one if the respondent is unemployed and fixed effects for highest educational attainment. All variables are standardized to *z*-scores. *** $p < 0.01$, ** $p < 0.05$, and * $p < 0.1$.

1.5 Conclusion

Why is conformity to social norms in some societies more prevalent than in others? I tested a hypothesis grounded in the cultural evolution literature: The more individuals have been exposed to negative shocks that threaten their society's survival, the more they conform to social norms. While being costly to individuals in good times, conformity to social norms becomes particularly beneficial in the face of shocks because it promotes collective action and thereby increases a population's chances of survival.

I tested this hypothesis using data on the occurrences of various types of shocks, including three different types of natural disasters and major economic downturns. The results paint a coherent picture: Be it in the form of droughts, earthquakes, hurricanes, or major economic downturns, the experience of negative shocks strengthens the importance people place in conformity to the existing social norms. Conversely, individuals who have lived through periods of high economic growth value norm conformity less.

These results shed light on three separate issues. First, while we increasingly understand the evolutionary process that determines today's differences in norms and cultural traits, we know less about why some societies display substantially greater cultural variability than others. The evidence in this paper suggests that such heterogeneity within groups follows an evolutionary

logic, too. It is a function of a societies' historical exposures to collective negative shocks. Second, the paper provides a rationale for the rise in polarization and cultural backlash in recent years. If groups have different norms, shocks may lead to polarization between groups through their impact on norm conformity. Third, the article illuminates the prosocial effects of short-run shocks. By increasing norm conformity, shocks promote prosocial behavior if the most valued local norms are prosocial. Conversely, it is also possible that they foster antisocial behavior, i.e., greater intolerance toward homosexuality, if this is what the local norms prescribe.

Future research might be able to study the behavioral consequences of greater norm conformity in response to shocks, and whether shocks also increase the willingness to punish norm violators.

2 Political Threat and Propaganda: Evidence from the U.S. South[1]

Joint work with Sebastian Ottinger

2.1 Introduction

Inflammatory propaganda in mass media can play an important role in political outcomes and violence. This is particularly the case when autocratic elites resort to hate creating stories that target certain ethnic or religious minorities. For example, hate stories broadcast by government-backed radio stations persuaded Hutu individuals to join the killings of Tutsis in the Rwandan genocide and stirred anti-Jewish violence in Nazi Germany (Yanagizawa-Drott, 2014; Adena et al., 2015). While mounting evidence suggests that propaganda can have serious consequences, we know less about its determinants. Natural drivers of propaganda may be past or current crimes committed by members of the targeted group or an evil ideology among members of the elite in power. In this paper, we take a different perspective. We investigate the possibility that propaganda may be the result of deliberate strategy responding to political threat.

Political threat refers to the fear among members of a dominant group of giving up political control and resources to a minority group. The work of Blalock (1967) on minority-group relations suggests that when two groups coexist with unequal access to political resources, the dominant group will engage in a wide variety of methods, including propaganda, to secure its privileged access to those resources. Building on this insight, models of the 'supply of hatred' formalize the conditions under which political threat may be an important driver of hate creating propaganda. According to Glaeser (2005), if a minority group fully supports one of two rival parties, then the other party may resort to propaganda that stirs resentment against the minority group to prevent the majority of the electorate from voting for the opponent. This logic suggests that hateful propaganda is part of the toolkit of political actors who seek to divide diverse coalitions.[2]

The best evidence in support of this hypothesis comes from studies examining the dynamics of anti-Black propaganda in the U.S. South. Woodward (1955) describes a rise in anti-Black antagonism after the Civil War, which was "was furthered by a sensational press that played up and headlined current stories of Negro crime, charges of rape and attempted rape, and alleged instances of arrogance." Glaeser (2005) shows that the frequency of anti-Black articles in the *Atlanta Constitution* increased between 1870 and the early 1900s and fell afterward until after World War I. He also observes that these trends coincided with changes in the political landscape and, in particular, with the rise and fall of the People's Party, also known as the Populist Party.

[1] This chapter should be cited as Ottinger, Sebastian and Max Winkler (2020): "Political Threat and Propaganda: Evidence from the U.S. South", Mimeo.

[2] Models of social identities such as Shayo (2009) also generate this hypothesis.

The Populists were the first American party committed to redistribution from rich to poor. They sought support among poor farmers, regardless of race, and advocated redistributionist policies that would have disproportionately benefited the poor, including African Americans. Their alliance with black voters was crucial for the success in the 1892 elections and threatened the dominant position of the Democratic elites in the South.

In this paper, we test whether the relationship between political threat due to the emergence of the Populist Party and the use of anti-Black propaganda in the media is causal. Direct evidence has proved elusive for two reasons. First, a systematic empirical analysis requires measurement of propaganda in the media, and thus detailed information on media content. Such data sets have been unavailable until recently. Second, credible estimates of the effect of political threat require an estimation strategy that deals with the multitude of unobserved factors that may affect both political threat and propaganda. To make progress, we collect new fine-grained measures of anti-Black propaganda by accessing the full text of several hundred Southern newspapers over many decades, ranging from rural weeklies to big-city dailies. We measure propaganda by counting the frequency of the word "rape" in co-occurrence with the word "negro" on the same page relative to the total number of newspaper pages.[3] Since newspapers were the dominant mass media at the time and highly local in their readership, they are the ideal source to measure variation in the supply of propaganda at the local level and over time.

To identify the effect of political threat on the spread of anti-Black propaganda in newspapers, we exploit variation in the Populists' unexpected success in the 1892 presidential elections in a difference-in-differences setting. Specifically, the Populists' success varied from state to state and even between counties within states. Where they gained votes, the Populists posed a more salient political threat to the local Democratic elites, providing them with an incentive to turn poor white farmers against blacks by fanning racial fears and spreading hatred (Du Bois, 1935; Woodward, 1955). In our baseline analysis, we define that local Democrats perceived political threat if the Populists gained a non-zero vote share in their county in the presidential elections of 1892. We then compare newspapers from counties where Democrats experienced threat to newspapers from counties where they were not (first difference), before and after 1892 (second difference). Importantly, our strategy allows us to include newspaper fixed effects, which remove time-invariant newspaper traits, including newspaper ideology. We find that newspapers in counties under political threat see a statistically and economically significant increase in propaganda relative to newspapers in other counties after 1892. Importantly, we find this effect only in newspapers that endorsed the Democrats in the presidential elections, but not in newspapers affiliated with the Republicans, the Populists, or independent newspapers. The spread of propaganda remains significantly higher until the early 1900s and abates afterward. This decline likely reflects the collapse of the Populist Party in the years after the 1896 election, which reduced the political incentive for Democrats to stir racial hatred.

Identification in our difference-in-differences specification rests on the assumption of parallel trends: absent political threat due to the rise of the Populist Party, newspapers in counties where the Populists gained votes would not have experienced a differential increase in propaganda. To

[3]The keyword selection is guided by Glaeser (2005) who uses a similar combination of keywords in his analysis of anti-Black articles in the *Atlanta Constitution*. Anti-Black propaganda was often propagated through stories of attacks by Blacks on the White community, often involving allegations of rape.

assess the plausibility of this assumption in our context, we estimate a dynamic difference-in-differences specification. We find no evidence for a pre-trend. Before the 1892 elections, anti-Black propaganda did not change differentially in counties exposed to vs. counties not exposed to political threat.

Another obvious concern with our result is that local Populist vote shares are not random. Determinants of anti-Black propaganda that also correlate with the local presence of the Populist Party may violate the parallel trends assumption of the difference-in-difference strategy. In particular, the Populists were more successful in counties that suffered from the economic downturn in the 1880s and 1890s (Eichengreen et al., 2019). It is conceivable that this economic distress gave rise to differential dynamics in anti-Black sentiment. To address this concern, we flexibly control for the effects of differences in local economic conditions. Specifically, we include a broad set of socio-economic county characteristics, interacted with year dummies, as control variables. The result corroborates our finding: the Populist political threat increases the prevalence of propaganda in newspapers affiliated with the Southern Democratic elite.

We also provide evidence in support of the interpretation that the effect is driven by the *supply* of propaganda. First, our evidence suggests that it is unlikely that local demand for racist stories drives the effect. Newspaper fixed effects remove time-invariant differences in demand across newspapers. Moreover, we control for the county-level Democrat vote shares in 1892, interacted with year dummies. The vote share serves as a proxy for local demand for anti-Black propaganda, and the interaction with year dummies flexibly removes demand effects that vary over time. Again, the result is very similar, supporting the interpretation that the supply of propaganda plays an important role in our setting. The finding is also consistent with the evidence in Gentzkow et al. (2015), who demonstrate that the Reconstruction South was the only place and period in American history during which state-level politics significantly affected newspaper circulation and political affiliation. It is precisely this political control of newspapers that we build our analysis on, and that makes a supply-side interpretation plausible. Second, an increase in real rape crimes is unlikely to account for the effect. We replicate the analysis using the extent to which newspapers report about rapes unrelated to African Americans as outcome variable. The coefficient of this placebo test is statistically indistinguishable from zero and, if anything, points in the opposite direction.

In light of the theory, we expect that political threat was felt more strongly in places where the Southern Democratic elite had more to lose from the redistributionist policies that the Populists advocated. We probe into the heterogeneity of the effect of political threat on anti-Black propaganda using the average size of farms in counties as a proxy for white wealth. The result suggests that the effect is stronger in counties with larger farms, and the magnitude of the heterogeneity is large: a one standard deviation increase in farm size increases the effect by 31.5%. Moreover, the effect is stronger in counties with a larger population share of African Americans, where the threat may have been more salient. As a placebo test, we also examine the effect of the Populist Party on anti-Black propaganda in newspapers outside the Southern states, where few African Americans lived. The Populists thus competed without relying on the support of black voters. Using the same empirical specification, we find no evidence that Populism per se affected anti-Black propaganda in newspapers.

Finally, we document that the propaganda wave in Democratic newspapers is associated with electoral gains for the Democratic party in subsequent elections. In particular, anti-Black propaganda in the years 1892 - 1894 had a lasting impact on future voting outcomes, while we find no evidence for an association before and after this period. This finding suggests that the propaganda "worked".

In sum, our results suggest that Southern Democratic elites responded to the emerging Populist threat by spreading anti-Black propaganda in local newspapers and that the propaganda was politically successful: counties with a larger increase in propaganda see stronger gains for the Democrats by 1900.

Our findings contribute to several strands of literature. A series of theoretical papers have formalized the idea, which goes back to Machiavelli, that elites may find it optimal to use a divide-and-rule strategy to remain in power against challenges. In Acemoglu et al. (2004), kleptocratic elites bribe pivotal groups to undermine competing alliances; in Padró i Miquel (2007), the fear of being ruled by elites who favor a different group drives voters to accept rent-extracting policies by their elites, even as those reduce their welfare; and in Shayo (2009), rich elites may appeal to voters nationalist identity to implement less redistributive policies. Closest to our setting is Glaeser (2005), who studies the supply of hate stories by politicians and voters' demand for such stories. We provide robust empirical evidence that the Southern Democratic elites circulated hate stories in the primary mass media of the time to divide an alliance of black and white voters.

We further contribute to an empirical literature that studies the economics of persuasion (reviewed in DellaVigna and Gentzkow, 2010) and the short- and long-run consequences of propaganda (DellaVigna et al., 2014; Yanagizawa-Drott, 2014; Cantoni et al., 2017; Durante et al., 2019; Bursztyn et al., 2019). We exploit a historical setting where newspapers were the single dominant source of information, and demand effects are less powerful because newspaper markets were often under tight political control (Gentzkow et al., 2015). Our study shows that political threat can be an important determinant of propaganda.

Our study also adds to papers and books on the economic history of the Postbellum South, including racism and political repression of African Americans during the Reconstruction period (e.g., Du Bois, 1935; Woodward, 1955; Margo, 1982; Foner, 1997; Cook et al., 2018). Our findings suggest that the rise and fall of hatred of Blacks in the late 19th and early 20th century was a political response to the redistributionist Populist movement of the 1890s. By measuring anti-Black propaganda from newly digitized historical newspapers, we provide a new large-scale data source that, we hope, will be helpful to many researchers in the field.

Finally, we contribute to a vast body of work spanning the fields of sociology, psychology, political science, and economics that investigates the historical origins of racism in the United States. While recent contributions highlight the importance of deep factors in shaping local racist attitudes today (Acharya et al., 2018; Williams, 2019), our findings suggest that even a short-lived spike in politically motivated propaganda affected racial attitudes for decades to come.

The rest of the paper proceeds as follows: Section 2.2 briefly describes the historical background, the features of the rise and fall of the Populist Party, its political platform, and how it

created political threat for the incumbent Democratic Party in the South. Section 2.3 describes our newspaper data set and how we measure anti-black propaganda and political threat at the local level. Section 2.4 lays out the empirical strategy, discusses the identification assumptions, and presents the main results and robustness checks. Section 2.5 investigates the extent to which the increase in propaganda affected electoral outcomes in the following decades. Section 2.6 concludes.

2.2 Historical Background

Three key features of the rise and fall of the Populist Party in the late 19th century make it an ideal context to the effect of political threat on the spread of propaganda in the mass media. First, the Populists' success in the 1892 election was unexpected and varied at the local level, providing us with a natural experiment. Second, the Populists initially sought support among poor farmers, regardless of race, and advocated redistributionist policies that would have disproportionately benefited African Americans. This diverse coalition and the Populists' redistributionist policy demands map precisely into the conditions under which political threat may become an important driver of hate creating propaganda. Third, the historical account widely agrees that the incumbent Democrats perceived the Populists as a serious political threat to their dominant position in the U.S. South. This perception provided the Democratic political elites with an incentive to turn poor white farmers against blacks by fanning racial fears and spreading hatred. We now describe these points in detail.[4]

2.2.1 The Rise of the Populist Party

The rise of the Populist Party as a significant political force in the South was unexpected. The depression of the 1880s gave rise to several grass-root organizations of dissatisfied farmers that blamed deflationary monetary policies and the monopoly power of railroad companies for the dire economic situation of many farmers. Across the country, numerous local self-help groups sprang up. These groups met at national and regional conventions to discuss ways to influence policy by co-opting the major political parties. Yet, the formation of a new party was not the goal until the early 1890s as many Southern participants at these conventions opposed the idea.

> Led by Leonidas F. Livingston of Georgia, a number of southern delegates made it perfectly plain that they would never consent to any program that would threaten the unity of the white vote in the South and they promised to bolt the convention should such action be taken. To avoid disruption, therefore, the third party decision was waived and the convention devoted itself to the business of drawing up a satisfactory list of demands. (Hicks (1928))

[4]We purposefully restrict the scope of this section to the historical features that are key to our research question and the empirical analysis. Hicks (1931) and Goodwyn (1978) provide excellent histories of the Populist Party. Du Bois (1935), Woodward (1955), and Hahn (2003) trace the history of the African American political struggle in the U.S. Abramowitz (1953), Meier (1956), Shapiro (1969), and Saunders (1969) are excellent examples from a large literature discussing the political role of African Americans during the time of the the Populist party.

Overcoming this opposition, the Farmers' Alliance established a full-fledged party before the 1892 election, where the Populist candidate James Weaver won 8.5% of the national vote and garnered much support in the South. Figure B.1 displays the county vote shares in the South.

2.2.2 The Populists' Political Platform

The Populists advocated redistributionist policies. Their 1892 party program highlighted inequality as a major concern:

> The fruits of the toil of millions are boldly stolen to build up colossal fortunes for a few, unprecedented in the history of mankind; and the possessors of those, in turn, despise the republic and endanger liberty. From the same prolific womb of governmental injustice we breed the two great classes - tramps and millionaires.

Their demands included a graduated income tax, nationalization of the railroads, telegraphs, and postal system, and an eight-hour workday. To alleviate the debt burden of poor farmers, the Populists also called for reforms to monetary policies, including the free coinage of silver.

> The national power to create money is appropriated to enrich bondholders; a vast public debt payable in legal tender currency has been funded into gold-bearing bonds, thereby adding millions to the burdens of the people.

Moreover, particularly in their early years, the Populists catered to African Americans in the South. In many counties, African Americans even served as local candidates and were given a voice in the party organization. This catering to the African Americans was in part political arithmetic, in part reflection of an egalitarian conviction, and often both:

> I am in favor of giving the colored man full representation. (...) He is a citizen just as much as we are, and the party that acts on that fact will gain the colored vote of the South. (President of the Texas Populists, cited in Woodward (1981))

According to Du Bois (1935), the potential gains from building an alliance of white and black labor in the South were clear:

> White labor in the South began to realize that they had lost a great opportunity, that when they united to disfranchise the black laborer, they had cut the voting power of the laboring class in two. White labor in the Populist movement of the eighties tried to realign the economic warfare in the South and bring workers of all colors into united opposition to the employer.

However, Populist support for African Americans faded over time. Some Populists dropped their attempts to recruit Black voters and endorsed both anti-Black policies and racial hatred after 1900. Thomas E. Watson, the Populist nominee for vice-president in the 1896 Presidential election, is a case in point. He turned from an outspoken supporter of black enfranchisement in the 1890s into a white supremacist after 1900. However, these changes typically occurred after the 1890s, the period of our empirical analysis.

2.2.3 The Populist Threat

Southern Democrats perceived the alliance between Populists and Black as a critical threat to their dominant position in the South. According to the historical account, many Democrats responded by fanning racial hatred, often in the form of newspaper stories of attacks of Blacks on the White community. Their goal was to prevent African Americans from voting and scare poor whites of negro domination if the Populists were to take control.

> Alarmed by the success that the Populists were enjoying with their appeal to the Negro voter, the conservatives themselves raised the cry of 'Negro domination', and white supremacy, and enlisted the Negrophobe elements. (Woodward (1955))

In several states in the South, Democratic governments also enacted laws that effectively disenfranchised African Americans and poor Whites, the Populists' most important supporters.

At the national level, the Democrats managed to co-opt the Populist party by taking over some of their policy platforms. While this co-option led to the fall of the Populist party in national politics after the 1896 election, several local Populist organizations continued to be active into the early 1900s. For example, the Populists of North Carolina made it into government by forming a coalition with the Republicans and stayed in office until after 1900.

2.3 Data and Measurement

Our difference-in-differences empirical strategy compares the prevalence of anti-Black propaganda in newspapers from counties where the Populists were politically active in the 1892 presidential election to counties where the Populists did not receive any votes. This empirical strategy requires the measurement of propaganda in newspapers over time and county-level data on electoral outcomes. This section describes our data source for newspaper content, the method to measure anti-Black propaganda, spatial and temporal patterns in this measure, and the definition of political threat at the county level.

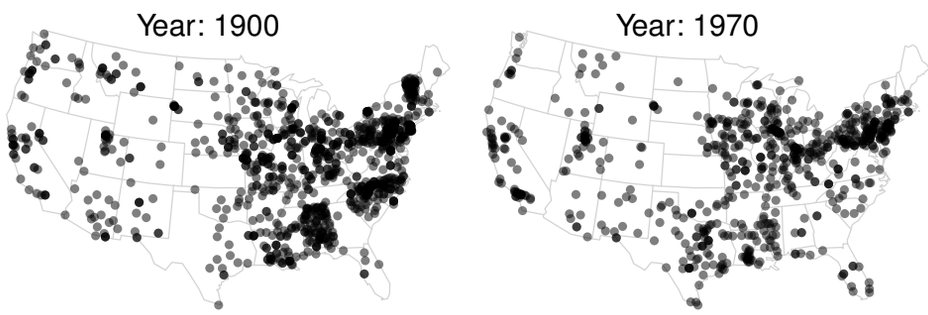
2.3.1 Newspaper Data

To investigate the occurrence of anti-Black propaganda across newspapers and over time, we draw on text data from *newspapers.com*, an extensive digital archive of historical and current newspapers. The provider scans newspapers and generates text using optical character recognition (OCR). The database is one of the most comprehensive digital newspaper archives currently available: it contains more than 550 million pages from over 17,000 newspapers – ranging from big-city dailies to rural weeklies.

We have developed an automated script that accesses the database via a personal subscription and downloads keyword frequencies. Specifically, we obtain the number of pages that a specified keyword appears on in a given newspaper and year. The script also allows us to search for co-occurrences of several keywords on the same page. We link the keyword frequencies to meta information of newspapers, including the place of publication for each newspaper recorded by *newspapers.com* and its longitude and latitude. Based on this information, we match each paper to a state and county.

Figure 2.1a displays the geographic distribution of newspaper locations in the data set over time. More than 1,300 U.S. counties have at least one newspaper at some point in the database. Importantly, the circulation of these newspapers was often highly local, typically limited to a single county. Thus, we interpret newspaper location as a proxy for newspaper coverage.

(a) Location of publication of newspapers in full database



(b) Southern counties with any newspaper between 1885 and 1903

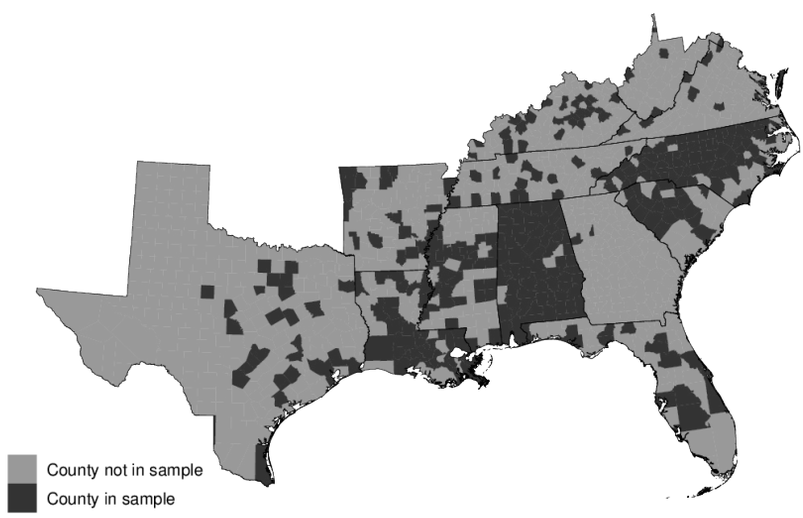


Figure 2.1: Geographic coverage of newspaper data set

Notes: Top panel: The map shows the locations of newspapers available from *newspapers.com* for 1900 and 1970. We exclude newspapers from Kansas because they are massively overrepresented in the database. Bottom panel: The map shows counties in the U.S. South for which we have newspaper data. Counties in dark (light) grey (do not) have newspapers at least once at any time between 1885 and 1903 and are (not) part of the analysis. Our coverage represents 42% of the population in Southern states.

The database comes with two shortcomings. First, it does not contain the universe of U.S. newspapers. When comparing the characteristics of counties with and without newspapers in

the database, we find that counties with newspapers are more likely to be urban, have a higher share of African Americans, and more manufacturing output per capita (unreported). Moreover, not all titles have a complete run of issues digitized. Some titles only have one issue, while others have thousands. This unbalancedness may cause problems for our estimation strategy if selective entry or attrition of newspapers is systematically related to our outcome and both differences. We will address these concerns in the analysis by assessing our estimates' sensitivity to different sample definitions.

Second, the database does not allow a search for keywords within specific types of newspaper content, such as editorials or letters to the editor. While it is impossible to read all of the content carefully, we verified the content of a random sample of 100 pages. Appendix Figure B.5 presents two examples.

For our empirical analysis, we restrict the database to newspapers published in the U.S. South between 1885 and 1903, the years around the presidential election of 1892. We are left with a sample of 764 newspapers in 329 counties, representing 42% of the population in the Southern states. Figure 2.1b displays the geographic coverage of the resulting data set. We obtain particularly good geographic coverage for the states of Alabama, Louisiana, and North and South Carolina.

2.3.2 Measurement of Anti-Black Propaganda

We measure anti-Black propaganda by implementing a word count exercise. For each year and newspaper, we count frequencies of the keywords "rape AND negro". In other words, we measure the presence of the word "rape" co-occurring with the word "negro" on the same page.[5]

To control for changes in the size of newspapers and coverage of the database, we also measure frequencies of the terms "monday OR tuesday OR wednesday OR thursday OR friday OR saturday OR sunday". We compute our measure of anti-Black propaganda as

$$\textit{Anti-Black Propaganda}_{i,t} = \frac{\sum_{n=1}^{N} n_{i,t} \times \mathbb{1}(\textit{rape AND negro})}{\sum_{n=1}^{N} n_{i,t} \times \mathbb{1}(\textit{weekdays})} * 100 \tag{2.1}$$

where n is the number of pages containing the keywords in newspaper i and year t. We multiply the resulting numbers by 100 to interpret *Anti-Black Propaganda* as the fraction of newspaper pages containing anti-Black propaganda in a specific newspaper and year.

Two issues with the measure are worth pointing out. First, the method of counting keyword frequencies on a page is dictated by our data source. The database structure prevents us from using more sophisticated methods to measure anti-Black propaganda in the newspapers.

Second, the resulting measure is a combination of reporting of (local and distant) rapes that occurred, their amplification by the local press, op-eds, letters to the editors, and fabrications. Building on the historical accounts, we argue that the bulk of the variation in the measure reflects *differential* reporting about local rapes, coverage of national headlines, op-eds, letters, and fabrications. We will come back to this point in the analysis. Specifically, we will provide evidence that the measure does not merely reflect the reporting of local rapes.

[5]The keyword selection is guided by Glaeser (2005) who uses the same combination of keywords in one of his analyses of anti-Black propaganda in the Atlanta Constitution.

2.3.3 Descriptive Analysis

What are the spatial and temporal patterns of anti-Black propaganda in US local newspapers? Figure 2.2 shows the geography of anti-Black propaganda, averaged from 1870 to 1965, using counties as the unit of observation. It shows the cross-county distribution of deviations from yearly averages, recovered as the residuals from regressing our propaganda measure on year fixed effects. Darker red colors indicate above-average anti-Black propaganda in a particular county, while darker blues indicate below-average values of anti-Black propaganda in newspapers of a county. No data are available for counties in grey.

The map reveals two striking features. First, there are pronounced differences across regions. Anti-Black propaganda is more common in the South as compared to the rest of the country. Particularly states within the South Atlantic census division, such as North and South Carolina, exhibit above-average values of propaganda. Still, it also holds for states in the East South Central census division, such as in Alabama and Tennessee. Second, the map shows that sizeable differences in anti-Black propaganda also exist within states, even among neighboring counties.

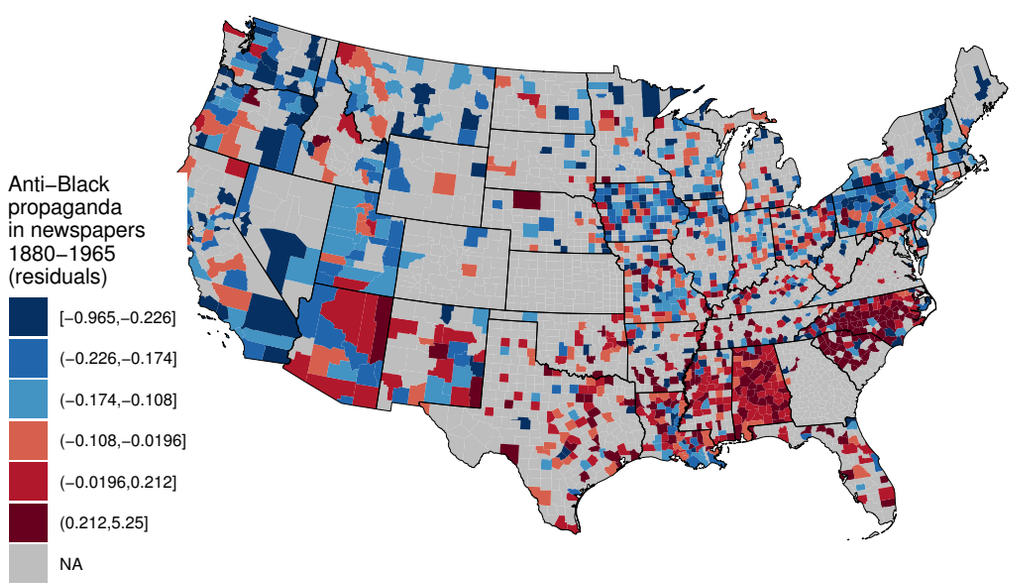


Figure 2.2: The geography of anti-Black propaganda in U.S. local newspapers.

Notes: This map shows the cross-county distribution of the residuals from regressing anti-Black propaganda from 1880 to 1965 on year fixed effects, as described in the text. Darker red colors indicate above-average anti-Black propaganda in a particular county, while darker blues indicate below-average values of anti-Black propaganda in newspapers of a county. No data are available for counties in grey.

Next, we investigate how regional differences change over time. Figure 2.3a shows the time variation by geographic region in the number of anti-Black propaganda in newspapers from 1880

to 1965. We document several interesting patterns. First, anti-Black propaganda markedly declined across all regions from 1880 to 1940; second, the South deviated from this long-term trend between 1880 and the early 1900s, which is the period of our analysis. Third, we see an increase in anti-Black propaganda after 1930 in the South and the Northeast, which becomes most pronounced in the South after 1940. Fourth, anti-Black propaganda was always most frequent in newspapers in the South, especially in the first 40 years of our sample period, and particularly so from 1880 to 1900. After 1900 a decade-long convergence to the lower levels of the Northeast, Midwest, and West sets in. Figure 2.3b depicts variation over time in anti-Black propaganda in local newspapers for rural and urban counties.[6] It shows that rural counties primarily drove the increase in anti-Black propaganda in the late 19th century, where Populists were particularly active.

To summarize, the raw data offers some preliminary evidence in support of the hypothesis. Deviating from a general decrease in anti-Black propaganda in US local newspapers, Southern and rural counties saw a short-lived spike in anti-Black propaganda between 1890 and 1900. Variation across Southern counties in this short spike will be the focus of our analysis.

2.3.4 Political Threat

The second key empirical challenge is to measure political threat due to the rise of the Populist Party at the local level. To this end, we collect data on electoral outcomes in the 1892 presidential election. For each county, we record the vote share of the Populist Party, provided by ICPSR (Clubb et al., 2006).[7] Appendix Figure B.1 depicts the Populist vote share in the 1892 Presidential election across counties in the US. It demonstrates that the Populists' electoral success varied from state to state and even between counties within states.

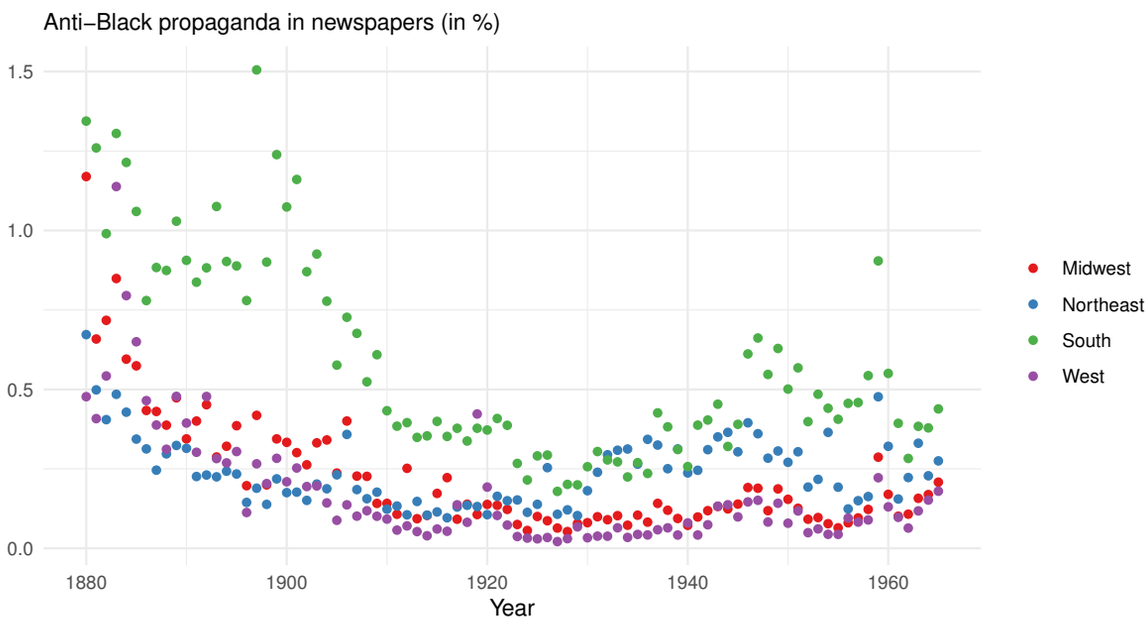
To operationalize political threat at the county level, we assume that where the Populists gained votes, they posed a political threat to the local Democratic elites. This assumption is motivated by the notion that what mattered to the local Democrat elites in their decision to "enlist the Negrophobe elements" (Woodward, 1955) was the *perceived* political threat resulting from the arrival of the Populists on the political stage, rather than their ability to attract a sizable vote share. We define an indicator for political threat, $\mathbb{1}(\textit{political threat}_c)$, equal to one if the Populist Party received any votes in the 1892 elections. Appendix Figure B.2 shows that this was the case in roughly 90% of counties, and Appendix Figure B.3 illustrates the counties presumed to be under threat for which we have newspaper data. Almost all states have at least one non-threatened county; however, most non-threatened counties are in Louisiana. Thus, we will assess the sensitivity of our results to excluding Louisiana from the sample as a robustness check.

We also test for the sensitivity of our results to alternative definitions of political threat. We explore whether our findings replicate in regressions that i) define the threat indicator equal to one if the Populist Party received a vote share higher than 10% in the 1892 elections, which

[6] We define rural counties as those with less than 200 persons per square mile in a given year. Yearly population density is calculated by linearly interpolating population from decennial censuses from 1880 to 1970.

[7] Populist vote shares for counties in Alabama are zero or missing in this data set. We draw on online sources to supplement these data.

(a) Anti-Black propaganda by census region



(b) Anti-Black propaganda in urban and rural counties

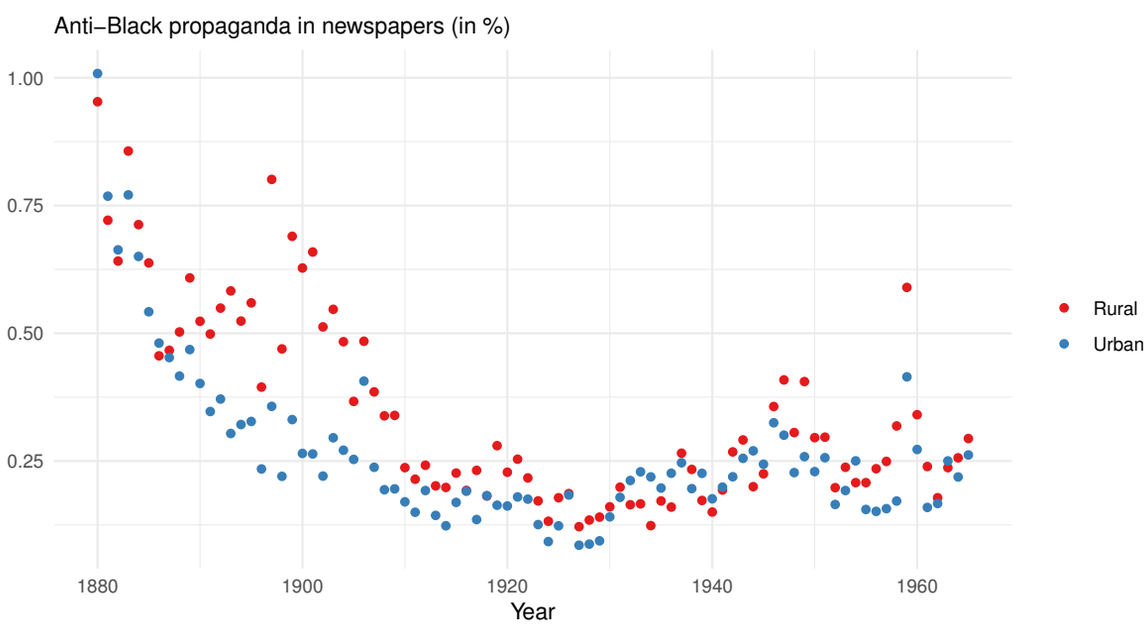


Figure 2.3: The evolution of anti-Black propaganda in U.S. local newspapers

Notes: This figure shows the time variation in the share of newspaper pages with anti-Black propaganda. In Panel (a), each dot corresponds to the population-weighted average level of anti-Black propaganda in a particular year in one of four broad geographic regions of the U.S. Panel (b) shows the population-weighted averages for rural and urban counties. We define rural as counties with less than 200 persons per square mile in a given year.

results in an approx. even split into threatened and non-threatened counties (see Appendix Figure B.4); ii) use vote share quintiles as main independent variable; and iii) include both the vote share and the baseline indicator as independent variables.

2.3.5 Other Data Sources

Newspapers at the time were often highly partisan and openly endorsed a particular party. Gentzkow et al. (2011) and Gentzkow et al. (2014) digitized newspaper directories that provide information about newspapers' political affiliations in presidential elections. We link this information to our newspaper data set to distinguish between newspapers that supported the Democratic Party and those that endorsed other parties or were independent. Running the analysis separately for Democrat and non-Democrat affiliated newspapers enables us to test whether all newspapers report more about rapes committed by Blacks after the Populist threat appeared in 1892 or whether this effect is limited to newspapers affiliated with the Democrats. We link endorsement in the 1892 election when such information is available. For newspapers that we cannot locate in 1892, we link the endorsement in the closest available year, i.e., in years 1896, 1888, 1900, 1884, 1904, and 1880 – in this order. Finally, we access county-level socioeconomic characteristics from the 1890 population census provided by Haines and Inter-University Consortium For Political And Social Research (2010) and the residential segregation index computed by Logan and Parman (2017)

2.4 Results

We now turn to the empirical analysis. In this section, we lay out the empirical strategy and present the results.

2.4.1 Empirical Strategy

We employ a difference-in-differences strategy with the first difference comparing the prevalence of anti-Black propaganda in newspapers from counties where the Populists gained votes in the 1892 presidential election to counties where the Populists did not receive any votes. The second difference compares changes in propaganda over time, in particular before and after the Populists arrived on the political stage in 1892. To this end, we define a dummy $\mathbb{1}(Post_t)$ that equals one from 1893 onward. We then investigate whether political threat is associated with an increase in anti-Black propaganda in newspapers by estimating the following regression:

$$\textit{Anti-Black Propaganda}_{i(cr),t} = \alpha_i + \alpha_{rt} + \beta\ \mathbb{1}(\ \textit{political threat}_c \times \mathbb{1}(Post_t) + \epsilon_{i(cr),t}. \quad (2.2)$$

The dependent variable $\textit{Anti-Black Propaganda}_{i(cr),t}$, defined in the previous section, is the measure of anti-Black propaganda in newspaper i in county c, census region r and year t. β is the coefficient of interest. If political threat increases the spread of propaganda, we expect that $\beta > 0$. Estimating regression (2.2) at the newspaper level allows us to control for time-invariant newspaper characteristics by including newspaper fixed effects α_i. This implies that

the identifying variation comes from changes *within* newspapers over time. We control for year ×census region fixed effects α_{rt} to remove variation that is year-specific across newspapers in the same census region (South Atlantic, East South Central Division, or West South Central Division). Standard errors $\epsilon_{i(cr),t}$ are clustered at the county-level, allowing for correlations of unobserved variation across newspapers in the same county and over time. Appendix Table B.1 provides summary statistics for all variables used in the analysis.

The central identifying assumption in our difference-in-differences framework is that of parallel trends in propaganda absent of treatment. In other words, absent political threat due to the rise of the Populist Party, newspapers in counties where the Populists won votes would not have experienced a differential increase in the spread of propaganda published therein. To inquire into differential trends, we implement a dynamic difference-in-differences estimation by estimating coefficients for each year separately. We then visually inspect yearly coefficients and test for the existence of a pre-trend in anti-Black hatred in newspapers across groups.

2.4.2 Main Result

Table 2.1 reports the results of the estimation of equation (2.2). We find a large and statistically significant relationship between political threat and the spread of anti-Black propaganda. The result in column 1 suggests that, after 1892, newspapers spread more anti-Black propaganda in counties where the Populist Party received a positive vote share in the 1892 presidential election. Since we include fixed effects for newspapers and year × census region, we identify the effect holding fixed newspapers' time-invariant racial bias and newspaper-invariant national and regional news affecting all newspapers in any given year and region. The effect size is large: relative to newspapers in counties with no political threat, we find a standardized effect size of 0.156 after 1892.

Next, we split the sample into newspapers that did not endorse the Democrats and those that did. Columns 2 and 3 report the results. Among 150 newspapers that were independent or affiliated with parties other than the Democrats, we find no increase in anti-Black propaganda after 1892. Instead, Democrat-affiliated newspapers drive the effect. For these, the estimate of β is 12% percent larger, while the standard errors only increase slightly by 10%, despite the smaller sample size. These results provide strong evidence supporting the narrative in Woodward (1955) that Democratic elites spread anti-Black propaganda in newspapers to discredit the Populists in the eyes of poor white voters. Democrat-affiliated newspapers in counties with political threat increased the spread of anti-Black propaganda with a standardized effect size of 0.175 after 1892 compared to Democrat-affiliated newspapers in counties without political threat.

Ruling out pre-trends. Our result could reflect differential trends in anti-Black propaganda that newspapers in counties with political threat followed already before the election in 1892. If so, the election result of 1892 could itself be an outcome of these differential trends, and the parallel trends assumption would be violated, which would invalidate our identification strategy. We conduct a dynamic difference-in-differences analysis to check for pre-existing trends in anti-Black propaganda. Figure 2.4 shows the coefficients of the regression of anti-Black propaganda on the political threat indicator interacted with year dummies, using the set of Democratic

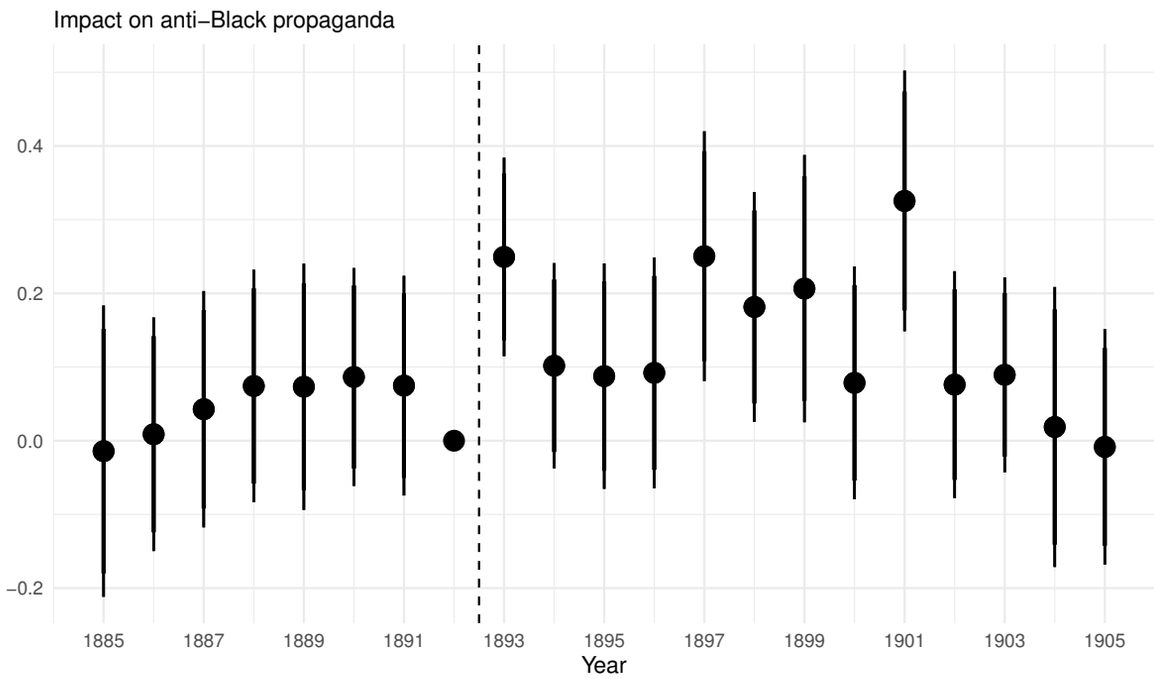
Table 2.1: Effect of political threat on anti-Black propaganda.

	Anti-black propaganda [std.]				
	All	Non Dem.	Democratic Newspapers		
	(1)	(2)	(3)	(4)	(5)
Political threat × Post 1892 [std.]	0.156***	0.008	0.175***	0.203***	0.166***
	(0.041)	(0.081)	(0.045)	(0.048)	(0.053)
No. of newspapers	764	110	654	654	654
Newspaper FE	Yes	Yes	Yes	Yes	Yes
Year-region FE	Yes	Yes	Yes	Yes	Yes
Economic conditions × year FE	No	No	No	Yes	Yes
Dem. vote share × year FE	No	No	No	No	Yes
Observations	5,399	666	4,733	4,733	4,733
R^2	0.502	0.602	0.496	0.525	0.530

Notes: This table shows that political threat due to the rise of the Populist Party increased the frequency of anti-Black propaganda in newspapers. An observation is a newspaper-year from 1885 to 1903. The outcome in each column is anti-Black propaganda in newspapers. The main independent variable is an indicator equal to one if the Populist Party gained votes in the presidential election of 1892 in the newspaper's county (first difference) interacted with an indicator equal to one for years greater than 1892 (second difference). All regressions include newspaper and year × census region fixed effects. Column 1 shows the estimate for the full sample. Column 2 restricts the sample to newspapers that do not endorse the Democratic Party Column 3 to 5 focus on newspapers that endorse the Democratic Party for which this information is not available. Column 4 adds county-level economic controls, measured in 1890, and interacted with year dummies. These controls include log population, black population share, residential segregation (in the year 1880), log per capita output in manufacturing and agriculture, the average farm size, log railway miles per square mile, the average indebtedness of farms (= mortgage value of farm/value of farms); the average interest rate on farms mortgages, the share of share-cropping farms, and the share of cotton acreage to total farm acreage. Column 5 adds controls for the vote shares for the Democratic Party in the 1892 presidential election, interacted with year dummies. All variables are standardized to z-scores. The standard errors are clustered on counties and reported in parentheses. ***, **, and * indicate significance at 1, 5, and 10 % levels.

newspapers and controlling for newspaper fixed effects and year × census region fixed effects as in Column 3 of Table 2.1.

Figure 2.4: Dynamic differences-in-differences analysis



Notes: This figure shows differences in anti-Black propaganda between newspapers in Democratic counties with versus without political threat in 1892, based on the specification of Column 3 in Table 2.1. It shows confidence intervals at the 95% (thin lines) and 90% (thick lines) level. Standard errors are clustered at the county-level. The F statistic for all coefficients before 1892 is 0.69 ($p = 0.68$).

We fail to detect a visible or statistically discernible pre-tend in anti-Black propaganda. The F statistic for all coefficients before 1892 is 0.69 ($p = 0.68$). Moreover, the graph shows that the effect vanishes after the Populist Party collapsed in the years after the 1896 Presidential election. This finding is in line with our interpretation of short-lived incentives to Democratic politicians to spread anti-Black propaganda in newspapers in places where the Populists threatened the Southern Democratic elites.

Differences in economic conditions do not drive the result. An obvious concern with our result is that the Populists' vote shares in the 1892 presidential elections are not random. Determinants of the local presence of the Populist Party that also correlate with anti-Black propaganda may violate the parallel trends assumption of the difference-in-difference strategy. For example, Eichengreen et al. (2019) show that the Populists were more successful in counties that suffered from the economic downturn in the 1880s and 1890s. It is conceivable that this economic distress gave rise to differential dynamics in anti-Black sentiment. In column 4, we address this concern by flexibly controlling for the effects of differences in local economic conditions in the years before 1890. Specifically, we include a large set of county economic characteristics, interacted with year dummies, as control variables: the log county population share, the county population share of African Americans, residential segregation, log per capita manufacturing

and agricultural output, average farm size, log miles of railways per square mile, average indebtedness of farms, average interest rates paid on farm debt, the share of cotton acreage to total farm acreage, and the share of sharecropping farms of the total number of farms. The latter two are motivated by the possibility that white plantation owners had an incentive to fan racial hatred to prevent black and white sharecroppers from joining forces and fight for higher wages. Column 4 shows that flexibly controlling for differences in these characteristics increases the coefficient to a standardized effect size of 0.203 while leaving the standard error almost unchanged. The result corroborates our finding: the Populist political threat increases the prevalence of propaganda in newspapers affiliated with Southern Democratic elites.

Controlling for differential increase in demand for propaganda. Based on historical accounts (Woodward, 1955) and the weak competitive forces in Southern media markets (Gentzkow et al., 2015), we argue that newspapers supply propaganda; that is, political actors such as newspaper editors, owners, and local officeholders were in the position to spread anti-Black propaganda in their newspapers. A competing view is that newspaper content is largely driven by readers' demand.[8] This view raises the concern that any increase in anti-Black propaganda after 1892 may be due to local demand for such content. While our newspaper fixed effects remove time-invariant differences in newspaper ideology and local demand, it may still be the case that differential shifts in demand over time may confound the result. To address this concern, we also control for the county-level Democrat vote share in the 1892 presidential election, interacted with year fixed effects. The vote share proxies local demand for anti-Black propaganda, and interaction with year dummies allows the demand effect to vary flexibly over time. Column 5 in Table 2.1 reports the result. The effect remains positive and highly statistically significant but loses roughly 18% of its effect size. Thus, the finding is in line with local preferences driving some of the demand for newspapers, but local demand plays a lesser role in our setting. Even with such a demanding specification, β is precisely estimated and sizable.

No differential increase in reporting of rapes unrelated to African Americans. In the previous section, we pointed out that our dependent variable reflects a combination of reporting of actual rapes (local and distant), their amplification by the local press, and op-eds, letters to the editors, and fabrications. This measurement raises the question of how to interpret the results; in particular, if the actual incidence of rapes increased in counties where the Populists entered local politics, our estimate of β could reflect accurate reporting. The best solution to this problem would be to control for the actual occurrences of rapes by using yearly crime statistics from primary sources with information on the type of crime and the race of the offender. Unfortunately, such data are not readily available, and would potentially still reflect biases in the local judicial system.

As an alternative solution, we conduct a placebo test, where we estimate the effect of political threat on the extent to which newspapers report about rapes unrelated to African Americans. To do so, we estimate such rapes' reporting by counting the occurrence of the keyword "rape" and

[8] Gentzkow and Shapiro (2010) show that for the U.S. from 1972 to 1998, demand for media slant, as revealed in local political vote shares, are a more important determinant of newspaper slant than is the identity of the ownership group.

subtract the number of times "rape" co-occurs with "negro" in local newspapers. We aggregate the frequencies to the newspaper-year level, normalize it by the measure for overall text length. Then, we replicate the previous regressions using the new outcome. Appendix Table B.7 shows that the coefficients of this placebo test are statistically indistinguishable from zero and, if anything, point in the opposite direction.

No effect outside the South, where political incentives to spread propaganda were absent. Finally, we examine the effect of the Populist Party on anti-Black propaganda outside the Southern states, where few African Americans lived. The Populists thus competed without relying on the support of black voters. In the Midwest, for example, the Populists' were hugely successful in the 1892 election – they carried entire states such as Kansas or Colorado – but their position on race was less salient. Thus, we expect that the Populist Party's presence did not create an incentive for white elites to spread anti-Black propaganda because there was no diverse coalition to split.

Table 2.2 reports the result of this placebo test. Using the same specifications as before, we fail to detect an effect outside the South. The coefficients are small and change signs between specifications. We therefore conclude that the Populist Party's presence did not affect the spread of anti-Black propaganda in non-Southern states. This finding provides another piece of evidence in support of the hypothesis.

In sum, our results suggest that political threat due to the rise of the Populist Party increased the spread of anti-Black propaganda in Democratic newspapers in the South. The effect is unlikely to be driven by shifts in factors related to economic characteristics or in demand for racist content, nor do we find evidence that real occurrences of crimes differentially increased. Lastly, we fail to detect an effect in places where the political incentives to spread anti-Black propaganda were generally absent.

2.4.3 Robustness

We now turn to the examination of the sensitivity and robustness of our baseline estimates.

Alternative definitions of political threat. So far, we assumed that a vote share greater than zero for the Populists created political threat for the Democratic elites. We now show that our findings replicate if we replicate our analysis using three alternative definitions of political threat. First, we define the political threat indicator equal to one if the Populists received a vote share higher than 10% in the 1892 presidential elections. According to this definition, the elites ca. half of the counties in our sample are assumed to perceive threat. Appendix Table B.2 shows that all the conclusions of the analysis go through. The effect sizes become slightly smaller, possibly because elites perceived threat even when the Populists gained a vote share of less than 10%. Second, we add the vote share for the Populist Party to the regression. Table B.3 shows that while the coefficient on Populist vote share is positive across specifications, the effect mainly comes from the political threat indicator. Third, we use quintiles of the Populist vote share as main independent variables. This definition allows us to examine whether a higher Populist vote share has a stronger effect on propaganda, arguably because it created a more

Table 2.2: Placebo: Non-Southern states.

	Anti-black propaganda [std.]					
	Northeast	Midwest	West	All regions		
	(1)	(2)	(3)	(4)	(5)	(6)
Political threat × Post 1892 [std.]	0.022	−0.050	−0.050	−0.007	0.029	0.023
	(0.022)	(0.043)	(0.043)	(0.029)	(0.032)	(0.033)
No. of newspapers	394	494	77	965	965	965
Newspaper FE	Yes	Yes	Yes	Yes	Yes	Yes
Year-region FE	Yes	Yes	Yes	Yes	Yes	Yes
Economic conditions × year FE	No	No	No	No	Yes	Yes
Dem. vote share × year FE	No	No	No	No	No	Yes
Observations	4,017	4,251	4,251	9,073	9,073	9,073
R^2	0.334	0.375	0.375	0.364	0.387	0.389

Notes: The table shows that the Populist Party's presence did not increase the frequency of anti-Black propaganda in non-Southern states. An observation is a newspaper-year from 1885 to 1903. The outcome in each column is anti-Black propaganda in newspapers. The main independent variable is an indicator equal to one if the Populist Party gained votes in the newspaper's county in the presidential election of 1892 (first difference) interacted with an indicator equal to one for years greater than 1892 (second difference). All regressions include newspaper and year fixed effects. Columns 1, 2, and 3 restrict the sample to states in the Northeast, Midwest, and West, respectively. Column 5 adds controls for county-level economic conditions in 1890, interacted with year dummies. These controls are described in Table 2. Column 6 adds controls for the Democratic Party's vote shares in the presidential elections of 1892, interacted with year dummies. All variables are standardized to z-scores. The standard errors are clustered on counties and reported in parentheses. ***, **, and * indicate significance at 1, 5, and 10 % levels.

salient political threat. Table B.4 reports positive and statistically significant coefficients for higher quintiles of Populist vote shares but not for lower quantiles. In sum, our main finding is robust to different definitions of political threat at the county level.

Balanced panel. The newspaper database is highly unbalanced. While some newspapers are available over many years, most newspapers are available for short periods only. An unbalanced panel may cause problems for our estimation strategy if the entry and attrition of newspapers are systematically related to our outcome and both differences. To deal with this concern, Appendix Table B.5 focuses only on the balanced panel of 60 newspapers from 1885 to 1903. With the substantially smaller sample, we obtain larger and highly statistically significant coefficients. This result lends empirical support to the assumption underlying our main result. Neither newspapers' selective entry or exit, nor their inclusion into the digitized sample drive the results. If anything, these factors work against us.

Dropping Louisiana. Table B.6 drops all observations from Louisiana, where no county voted for the Populist presidential candidate in the 1892 election because Democrats and Populists combined their electoral tickets in 1892 (White, 1918). This fusion constrains our ability to identify political threat at the county level in our election data. Nevertheless, the same issues that drove poor white and black voters to the Populists elsewhere were also at work in Louisiana.[9] Therefore, we are concerned that including Louisiana and implicitly assuming that the Populists received zero vote share across counties introduces a downward bias, as we expect that Democrat elites in Louisiana also resorted to propaganda to respond to the Populist threat. Reassuringly, Table B.6 shows that our estimates barely change when we drop Louisiana from the sample.

2.4.4 Heterogeneous Effects

We now probe into the heterogeneity of the effect. First, we explore whether the effect size varies based on pre-existing wealth differences among Whites. In light of the theory, we expect that white elites felt more threatened when they had more to lose from the redistributionist policies that the Populists advocated. We proxy white wealth by the average sizes of farms in counties. Column 1 in Table 2.3 reports the result. We find a positive and statistically (marginally) significant coefficient on the interaction term. The magnitude of the coefficient on the interaction term is large: a one standard deviation increase in farm size is associated with a more than 31% stronger effect of political threat.

Next, we examine whether the effect was stronger in rural than in urban counties. The Populists sought support among poor farmers. We, therefore, expect that elites in rural counties perceived more threat than in urban counties. Column 2 reports a negative coefficient on the

[9] According to (White, 1918): "By entering into the fusion agreement, it was asserted, the people's party was merely fighting the democrats with their own methods. In concluding, an appeal was made to the voters to have the manhood to assert their rights, not to let the scarecrow of negro domination longer drive them to the democratic wigwam, and to rally to the standard of the people's party and elect the fusion ticket as a re buke to 'the Democrats in their strength, and the Republican party in its weakness. May Louisiana break the 'solid south' and greet our great toiling brethren of the North and West with the cheering hope of industrial reform in the near future."

interaction between political threat and log county population; however, the coefficient is not statistically significant at conventional levels.

We also assess whether the effect is stronger in places with a larger share of African American residents or in more segregated counties. Columns 3 and 4 show a positive coefficient on the interaction with the county population share of African Americans and a negative coefficient on the interaction with residential segregation. This result suggests that the effect of political threat on propaganda is more substantial in places with more African Americans and where African Americans were more likely to live next to Whites. These findings are consistent with the possibility that the perceived threat was more serious when African Americans were more salient to white residents.

Table 2.3: Heterogeneity in the effect of political threat on anti-Black propaganda.

	Anti-black propaganda [std.] Democratic Newspapers				
	(1)	(2)	(3)	(4)	(5)
Political threat × Post 1892 [std.]	0.181***	0.180***	0.178***	0.174***	0.189***
	(0.044)	(0.045)	(0.046)	(0.044)	(0.046)
× Avg. farm size [std.]	0.057*				0.061*
	(0.032)				(0.033)
× Log population [std.]		−0.024			−0.010
		(0.028)			(0.027)
× Share black pop. [std.]			0.019		0.049*
			(0.020)		(0.029)
× Residential segregation [std.]				−0.015	−0.040
				(0.022)	(0.027)
No. of newspapers	654	654	654	654	654
Newspaper FE	Yes	Yes	Yes	Yes	Yes
Year-region FE	Yes	Yes	Yes	Yes	Yes
Observations	4,733	4,733	4,733	4,733	4,733
R^2	0.497	0.496	0.496	0.496	0.498

Notes: This table shows that the effect of political threat on anti-Black propaganda is stronger in places with larger farms and a larger population share of African Americans. An observation is a newspaper-year from 1885 to 1903. The outcome in each column is anti-Black propaganda in newspapers. All regressions include newspaper and year-census division fixed effects. The sample is restricted to that endorse the Democratic Party and for which this information is not available. All variables are standardized to z-scores. The standard errors are clustered on counties and reported in parentheses. ***, **, and * indicate significance at 1, 5, and 10 % levels.

2.5 Did the Progaganda Affect Voting?

Our findings provide insights into a so-far untested determinant of propaganda. Since previous studies have found that propaganda can affect behavior, the question arises whether, in our

context, the propaganda "worked". Did it sway people to vote for the Democrats? To investigate this question, we examine whether anti-Black propaganda during the 1890s is associated with electoral outcomes in subsequent elections. Specifically, we test if counties see stronger electoral gains for the Democrats in the early 20th century if Democratic newspapers spread more anti-Black propaganda during the 1890s. We estimate the following equation,

$$Dem.\ Vote\ Share_{c,\ 1900,\ ...,\ 1916} = \sum_{t=1885}^{1900} \beta_t\ propaganda_{i(c)t} \times D_t + Dem.\ Vote\ Share_{c,\ 1892} + X_c'\gamma + \varepsilon_{i(c)t}$$

where $Dem.\ Vote\ Share_{c,\ 1900,\ ...,\ 1916}$ and $Dem.\ Vote\ Share_{c,\ 1892}$ denote the county-level vote shares for the Democratic Party in years 1900, 1904, 1908, 1916 and 1892, respectively; $propaganda_{i(c)t}$ captures the prevalence of anti-Black propaganda in local newspapers; D_t is an indicator variable for each year; X_c' denotes a vector of region fixed effects and the same county-level socioeconomic characteristics that we use and describe in the main analysis (Section 2.4.2); and ε_c is the error term. Standard errors are clustered on counties. The coefficients of interest are β_t, in particular for the years after 1892, when the Populists threatened Southern Democrats.

Table 2.4 reports the results. In most years after 1892, it shows a positive association between anti-Black propaganda in Democrat-affiliated newspapers and the Democratic vote share in future elections. Moreover, the relationship is highly statistically significant for propaganda in 1893, when Southern Democrats perceived the Populist threat for the first time. Thus, we find suggestive evidence that the propaganda was politically successful: counties with a larger increase in propaganda see stronger gains for the Democrats in presidential elections by 1900.

Table 2.4: Effect of anti-Black propaganda on future Democratic vote share.

	1900	1904	1908	1912	1916
	Democratic vote share in year [std.]				
	(1)	(2)	(3)	(4)	(5)
Anti-black propaganda [std.] × Year 1886	0.036	−0.004	0.007	0.005	−0.013
	(0.043)	(0.033)	(0.029)	(0.033)	(0.045)
Anti-black propaganda [std.] × Year 1887	0.052**	−0.004	−0.004	0.006	0.008
	(0.022)	(0.019)	(0.020)	(0.021)	(0.019)
Anti-black propaganda [std.] × Year 1888	0.065***	0.019	0.027	0.037*	0.015
	(0.025)	(0.020)	(0.020)	(0.022)	(0.022)
Anti-black propaganda [std.] × Year 1889	0.055***	0.010	0.020	0.027	0.015
	(0.020)	(0.018)	(0.017)	(0.020)	(0.019)
Anti-black propaganda [std.] × Year 1890	0.031	−0.001	−0.006	0.000	−0.002
	(0.026)	(0.017)	(0.020)	(0.020)	(0.024)
Anti-black propaganda [std.] × Year 1891	0.028	0.008	0.016	0.014	0.017
	(0.025)	(0.024)	(0.027)	(0.027)	(0.031)
Anti-black propaganda [std.] × Year 1892	0.064*	0.038	0.033	0.044	0.064**
	(0.037)	(0.033)	(0.036)	(0.033)	(0.028)
Anti-black propaganda [std.] × Year 1893	0.076***	0.042**	0.061***	0.066***	0.051***
	(0.025)	(0.018)	(0.018)	(0.019)	(0.018)
Anti-black propaganda [std.] × Year 1894	0.083**	0.045*	0.045	0.062**	0.041
	(0.036)	(0.027)	(0.030)	(0.030)	(0.028)
Anti-black propaganda [std.] × Year 1895	0.042	0.031	0.017	0.011	0.002
	(0.026)	(0.022)	(0.021)	(0.027)	(0.027)
Anti-black propaganda [std.] × Year 1896	0.034	0.010	0.019	0.018	0.012
	(0.031)	(0.028)	(0.025)	(0.031)	(0.027)
Anti-black propaganda [std.] × Year 1897	0.012	−0.001	−0.003	−0.003	−0.014
	(0.015)	(0.013)	(0.013)	(0.016)	(0.015)
Anti-black propaganda [std.] × Year 1898	0.041*	−0.003	−0.005	0.007	−0.008
	(0.022)	(0.018)	(0.018)	(0.019)	(0.017)
Anti-black propaganda [std.] × Year 1899	0.039**	0.013	0.017	0.017	0.007
	(0.016)	(0.013)	(0.013)	(0.015)	(0.013)
Anti-black propaganda [std.] × Year 1900	0.024	0.013	0.011	0.011	−0.001
	(0.022)	(0.015)	(0.015)	(0.017)	(0.015)
No. of counties	276	276	276	276	276
Region FE	Yes	Yes	Yes	Yes	Yes
Economic conditions	Yes	Yes	Yes	Yes	Yes
Dem. vote share 1892	Yes	Yes	Yes	Yes	Yes
Observations	3,578	3,587	3,587	3,587	3,587
R^2	0.674	0.716	0.700	0.628	0.636

Notes: This table shows that anti-Black propaganda after 1892 affected the Democrat vote share in subsequent presidential elections. An observation is a newspaper-year from 1886 to 1900. The sample includes newspapers that endorse the Democratic Party and for which this information is not available. All regressions include census region fixed effects, county-level economic controls, which are described in Table 1, and the vote share for the Democratic Party in 1892. All variables are standardized to z-scores. The outcome in each column is the vote share for the Democratic Party in presidential elections in the years 1900 - 1916. The standard errors are clustered on counties and reported in parentheses. ***, **, and * indicate significance at 1, 5, and 10 % levels.

2.6 Conclusion

This study provides empirical evidence for the hypothesis that autocratic political elites resort to hate creating propaganda when a diverse coalition threatens their dominant position. We exploit the rise of the redistributionist Populist party in the presidential 1892 election and the threat they posed to Southern Democratic politicians by aligning the interests of white and black poor farmers. We find that newspapers fanned racial hatred aimed at preventing poor Whites from voting for the Populists.

The empirical analysis makes use of a novel measure of anti-Black propaganda based on text data from an extensive corpus of newspapers. In a difference-in-differences framework, we show that newspapers in counties where the Populists received votes in the 1892 presidential election spread more anti-Black propaganda in the following years compared to newspapers in counties where the Populists did not pose a threat. Our results are identified from within-newspaper variation driven by newspapers affiliated with the Democrats, lending support to our interpretation that the effect is due to the supply of propaganda. The evidence also suggests that the effect is not present outside the South, where the political incentive to spread anti-Black propaganda was absent. Moreover, the effect is neither due to an increase in demand for such content nor due to differences in economic conditions. Finally, we find evidence suggesting that a rather short period of anti-Black propaganda shaped political outcomes for decades to come.

3 Traditional Belief Systems and Economic Behavior: Evidence from Beer Retailers in the Eastern DRC[1]

Joint work with Raul Sanchez de la Sierra

3.1 Introduction

Traditional religious beliefs – such as beliefs in supernatural powers or ancestral spirits – are widespread today and permeate the historical account. The average number of people who report believing "witchcraft" or the "evil eye," two common examples of traditional supernatural beliefs, is shown visually in Figure 3.1. While the beliefs can be found across the globe, they are especially strong in sub-Saharan Africa. Despite the near-universal adoption of Islam and Christianity, traditional practices linked to ancestral supernatural powers remain common. Several countries report rates that are well over 90% in all parts of the country (Gershman, 2016).

Despite such statistics, the true strength of these beliefs is not known. More importantly, even if these beliefs were strongly held, we do not know the extent to which they affect psychological well-being, economic choices, and ultimately, economic growth. This issue is particularly important for the field of economic development. While such traditional religious beliefs are well-recognized and widely studied in other fields, such as anthropology and history, within economics they have not been widely integrated into our understanding of human behavior, whether it be social or economic.[2]

In this paper, we randomly supply protection rituals that provide a supernatural protection spell to a group of 35 beer retailers from rural villages in the Eastern Democratic Republic of the Congo (DRC). Eastern DRC provides a suitable testing ground to examine the prevalence, and relevance, of beliefs in non-falsifiable ancestral supernatural forces. First, the region experiences high insecurity of property rights, and as a result the retailers' main concern is that their beer stocks may be subject to theft, often at gunpoint. As a result of the perceived insecurity, beer retailers reduce their levels of beer inventories, and frequently run stock-outs, limiting their ability to sell. Second, as in most sub-Saharan Africa and much of the world, there are diverse forms of supernatural protection against bad events. Within this area the magic is called *grigri*. The spells generally involve a ritual, an enchanting of an object, and the requirement that

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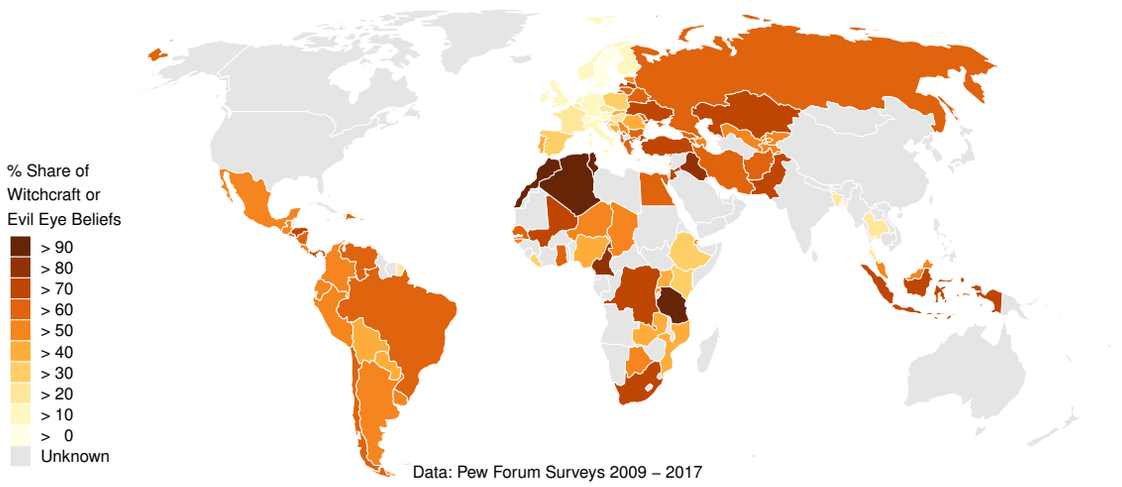
[2] There are a number of important exceptions to this statement. For pre-existing scholarship within economics on the consequences of traditional belief systems within sub-Saharan Africa, see for example Gershman (2015), Gershman (2016), Alonso et al. (2016), Nunn and Sanchez de la Sierra (2017), Alidou and Verpoorten (2019), and Stoop and Verpoorten (2020).

specific conditions be followed for the protection to remain in place.

Figure 3.1: Prevalence of beliefs in witchcraft or the evil eye



Notes: The map displays nationally representative data collected by the Pew Forum in several survey waves between 2009 and 2017. The surveys asked respondents to indicate (*i*) whether they believe in witchcraft, and (*ii*) whether they believe in the evil eye or that certain people can cast curses or spells that cause bad things to happen to someone. In Western European countries, the survey asked the evil eye but not the witchcraft question. We calculate the country-level shares of individuals who believe in witchcraft or the evil eye (or both) using the sampling weights that come with the survey data.

We consider anti-theft protection rituals performed by reputable witch doctors of the area. Anti-theft protection spells, which are provided through the hour-long in-person rituals, it is believed, reduce the likelihood that beer inventories will be subject to theft. During the ritual, the witch doctor invokes the ancestors of the tribe, communicates with them the desire that they protect his subject, and performs a ceremony that often involves chants, dance, and a variety of animal and vegetable products applied either to the subject or to an object. At the end of the ritual, an object is provided that embeds the newly acquired protective powers. The powers are expected to work as long as the object is placed in the store and certain necessary conditions (i.e., actions) are followed.[3] The rituals were provided by three different witch doctors who were from three different regions, which we identified following thorough background checks aimed at getting the most reputable among legitimate witch doctors.

We use an administrative dataset that provides detailed retail information at the day level. We randomize the exact date at which a beer seller receives a ritual, which allows us to exploit within-individual variation over time as well as between individual variation. To causally estimate the effect of receiving supernatural protection, we compare 'treated' observations (days when a beer seller is protected) with 'non-treated' observations. Using this design, we estimate the effects of the rituals, and the protection that they provide, on economic and psychological outcomes of the protected beer sellers and examine the dynamics of such effect.

We find that, for the retailers who believed in the spell, the ritual reduces their self-reported perceived risk of theft. For these retailers, the ritual also increases product purchases and their

[3]In all three regions, the object protected against theft, while in Walikale, the object was also promised to provide good luck, which would result in increased sales. In Walikale the ritual offered could not separate protection from luck.

average stock of inventory, effects that are consistent with their perception of lower probabilities of theft. As a consequence, retailers are less likely to have stock-outs, resulting in increased sales, revenues, and profits. By contrast, we find no such effects for the retailers who did not believe that the rituals worked.

The economic and social benefit of the rituals raises the question of why every beer seller did not already have a protection spell. From focus groups, the primary reason appears to be cost. Traditionally, the cost of this particular *gri-gri* was one goat. Today, the monetary equivalent (approximately $40–50) is also accepted. Thus, the *gri-gri* can be viewed as a valuable investment to make if one has the liquidity to pay for it, similarly to schooling or hospital visits. Thus, some purchase and some do not. Within our sample, we find that before we provided the protection spell, some individuals – 13 of 35 (37%) – had purchased a protection spell at some point in the past two months. According to our estimates, the average increase in profits during our sample period is $14 after two weeks, $40 after one month, and $61 after two months.

Our findings contribute to a number of literatures from different fields. First, outside of economics, the role that superstitions, rituals, and religious beliefs play in helping individuals cope with stress, uncertainty, or trauma has been long-recognized. However, studies tend to either be descriptive case studies (e.g., Malinowski, 1948; Sosis and Handwerker, 2011) or based on cross-sectional correlations (e.g., Bryant-Davis, 2013). One of the best-known studies of this type is Malinowski's (1948) study of the Trobriand Islanders of Melanesia. In the study, Malinowski documents their magic and rituals, and argues that one tends to observe the presence of magic when there is uncertainty. In his view, magic was a way to help the Trobriand Islanders cope in the face of uncertainty. He provides the example of fishing on the island. There were two types of fishing: lagoon fishing, which occurs in the calm inner waters, and deep-sea fishing, which occurs in the dangerous open ocean. Malinowski documents that rituals and magic were associated with deep-sea fishing but not lagoon fishing. Deep-sea fishers performed elaborate magical rituals to help ensure a safe trip and good results (Malinowski, 1948). Our findings contribute to this literature by providing causal estimates of the effect of such rituals on psychological well-being and economic decision-making, and thus provide a first experimental test of Malinowski's hypothesis as well as whether rituals have economic consequences.

Second, the findings contribute to existing laboratory studies that attempt to detect the existence of false beliefs. Augenblick et al. (2016) use a lab-based experiment to test the strength of a religious group's belief in the ending of the world in 2011, a belief that may be ex-ante false (and most likely is false). Consistent with their setting, individuals in our study report to have strong beliefs in non-falsifiable supernatural protection, and, consistent with their findings, we find that individuals who report having such beliefs change economic decision-making in accordance with such beliefs. However, our study's scope differs in a number of ways. First, behavior in the lab, where monetary incentives are often relatively small relative to individuals' income, are subject to self-image concerns and cognitive dissonance. For instance, individuals may be willing to pay a small cost so that their behavior aligns with the beliefs they are would like to have, even if they do not truly hold such beliefs. Our study addresses this concern by examining behavior related to the main source of income. The economic costs that individuals in our sample are willing to undergo would imply implausibly high self-image concerns if they

were driven by self-image concerns. Second, our study is focused on real-life decision making, in a way that allows us to forgo external validity concerns of the lab, and also compute the returns on investment to obtain such a ritual.

Finally, our findings are particularly important for today's policy in many countries. When engaging with non-Western societies, Western governments and NGOs frequently develop policies aimed at discouraging traditional superstitions and rituals. For example, in 2015 the Tanzanian government banned witchdoctors, citing the reports of albino killings undertaken by the doctors (Smith, 2015). Second, traditional rituals are explicitly discouraged by many governments because they are often perceived as forces that governments cannot control, and that potentially diminish their ability to govern. In the DRC, for instance, protection rituals are discouraged by the Congolese government and military, in part, because it is seen as a threat to the authority of the state. Given such efforts to reduce participation in traditional rituals, it is important to understand the consequences, whether they be negative or positive, of such practices. If they serve certain functions or have certain benefits, then it is important to understand these before policies that discourage or outlaw such practices are implemented. Our study provides a humble step in that direction, quantifying the partial equilibrium marginal psychological and economic benefits from the rituals.

Section 3.2 provides the context. Section 3.3 presents the details of the intervention. Section 3.4 presents the strategy to measure outcomes. Section 3.5 presents the empirical strategy to produce causal estimates. Section 3.6 presents the estimated effects of the rituals. Finally, Section 3.7 concludes.

3.2 Context

Before turning to our experiment, we first describe the setting and context of its environment.

3.2.1 Rural Nord Kivu

We study beer retailers in Nord Kivu, a province of Eastern DRC. Although nearly 100% of the population reports being Christian or Muslim, the use of traditional magic is widespread. Rituals that protect against theft or that helps to increase sales are particularly prevalent among our sample's population.

Beliefs about supernatural protection are present in all strata of society, having become more widely used with the rise in conflict in the region in recent decades. The region has experienced persistent insecurity and conflict since the Rwandan Genocide of 1994, when Hutu militia fled Rwanda into this area. It was the epicenter of the First Congo Civil War (1996–1997) and the second Congo Civil War (1998–2003), and dozens of militant groups, especially uncoordinated armed bandits, continue to operate today in this region, regularly engaging in violence, taxation, and often unpredictable theft. In this environment, retailers and households are exposed to weak property rights protection. In our baseline data, theft occurs in between 2–5% of days; in other words, theft occurs, on average, once every 20 to 50 days for the typical beer seller.

3.2.2 Beer distribution in rural Nord Kivu

Beer sellers are a particularly well-suited population to examine the economic consequences of insecurity and beliefs about property rights protection.

The distribution of beer in the DRC is split into two major supply chains. On the one hand, there is the firm Bralima, which is owned by Heineken. Bralima owns six breweries across the country and is estimated to control two-thirds of the country's beer market. While Bralima has no brewery in North Kivu, daily boats ship large volumes of its beers from its brewery in Bukavu (the capital of Sud Kivu) to Goma (the capital of Nord Kivu). From there, it is distributed in Goma and through the rural areas of North Kivu. Bralima bottles and distributes several beer varieties, including the famous Heineken beer and a popular Congolese beer named *Primus*. On the other hand, there are other distribution networks tied to a French consortium. The beers they sell are distributed through several areas of North Kivu and compete with the beers distributed by Bralima.

From its wholesalers in Goma and other provincial towns, the beer is then distributed to retailers. There are approximately 100 urban retailers and 150 rural retailers in North Kivu alone. Retailers sometimes receive the beer shipments directly from wholesalers. Retailers in certain locations must travel to the wholesaler to replenish their stock, which exposes them to theft along the road. Some retailers have exclusivity clauses with the firm behind their wholesaler, allowing them only to trade beer from that firm, and fixing the price at which they are allowed to sell (a practice known as retail price maintenance). These retailers often obtain, in exchange, an initial investment as well as marketing material from their respective firm. The other retailers are free to trade beer from any firm. The firms conduct ghost consumer audits to ensure that the price offered is consistent with the retailer contract. Such audits are especially easy to implement in cities, leading to Bralima's belief that deviations are frequent in rural areas.

Beer retailers sell the beer to local populations. Beer is one of the few commodities that is, at the same time, valuable and yet widely sold. One bottle of beer typically costs 1,500FC, which is about US $1.10. Bottles are reused, and the value of the bottle itself is about ten times the cost of a beer. While the urban retailers of Goma are often subject to informal taxation by state officials, it is common for beer retailers in the rural areas to be sacked by roaming militia, state soldiers, or robbed by a large number of roving bandits.

Because beer must be sourced from a small number of breweries, one of the primary decisions facing beer retailers is how much inventory to stock. This decision involves a trade-off between having more inventory, which reduces the risk of stock-outs, but increases the cost if stocks are stolen. Smaller inventory increases the risk of stock-outs but reduces the expected losses in the presence of weak property rights protection. Furthermore, beer sellers in rural areas have very low capacity, and thus often have poor management practices. For example, when the administrative firm-level data system was created, many beer sellers did not keep accurate accounting.

The data set contains rural beer retailers who, since 2015, have been reporting information daily. Each beer seller is from a randomly selected village within the area. Several aspects of beer retailers make them a particularly well-suited population to study. First, since purchases of beer

inventory can only be made from a limited number of retailers and only during a limited number of points in time, unlike for retailers of other goods, for beer sellers managing the purchase of inventories is a particularly important decision. Second, beer bottles are valuable and prone to theft. This is much less true for other commonly sold commodities, which are not held in as large of quantities and not as valuable. Thus, although there is nothing special about the use of rituals and amulets by beer retailers, beer retailing offers a setting for which we can most clearly identify a purpose for the rituals and amulets, and the plausible economic consequence of them. Third, the different beer varieties sold are identical across beer sellers, which allows for comparability across villages. Furthermore, most beer sellers sell predominantly the Primus beer, which allows us to compare subjects of the experiment with minimal extrapolation issues due to subject heterogeneity.

3.3 Intervention: The protection ritual

The protection and luck rituals that we provide are common in the region. However, the price of such rituals ($40–50) means that they are out of reach for most individuals most of the time. The average per capita GDP in the DRC is about $250. Thus, the price of protection is equal to about 2–3 months' worth of income.

In the survey we did to recruit beer sellers for this study, we asked them their maximum willingness to pay for the anti-theft ritual that we later offered them for free.[4] The average is $40.

In March of 2015 and again in June of 2016, the authors traveled to the region and organized a number of focus groups and pilot studies to gain the necessary background information about these rituals. We found that protection rituals, in general, permeate all levels of society. Most believe in their effectiveness, including those in the population who are university educated.

Although the finer details of protection spells vary from village to village, and even from witchdoctor to witchdoctor, they do share common features. The spells require a ritual that must be performed by someone with the knowledge and magical power necessary to endow such. The rituals are performed in private. In addition, the protection spells require certain conditions be met for the protection to continue working.

The intervention and data collection were approved by the Institutional Review Boards (IRB) of Berkeley (Protocol: 2016-06-8849; FWA: 00006252), Chicago (Protocol: IRB20-0927), and Harvard (Protocol: SITE18-0356; FWA: 00004837).

3.4 Measurement

To measure outcomes, we collected retail-level administrative data from 45 beer retailers from the regions of Masisi, Rutshuru, and Walikale. In 2015, a team of surveyors visited a random sample of villages in each region. In each village, if at least one beer retailer was present, then, after obtaining consent, the beer retailer was given a smartphone and a solar charger. We programmed an app so that the retailer could enter the details of his accounting every day. A

[4]Specifically, we asked *Up to what amount would you be willing to pay for the anti-theft gri-gri?*

the end of each week, the retailer was given the necessary phone credit and transport money to travel to the closest area with cellular coverage and upload the data from the previous week. The retailers received extensive training over multiple days. We also set up a hotline where, if there were technical issues with any of the equipment or software, the retailers could request a local supervisor to fix the problem. Finally, the smartphone recorded the GPS location where the data were entered.

To ensure data quality, the was specifically designed to be user friendly. Moreover, we monitored and checked the data monthly to ensure accuracy. We reported missing data or unusual entries to the specific beer seller's local supervisor, who then contacted the beer seller. The most common reason for missing entries was limited cell phone coverage which caused difficulties in sending the recorded data. At the end of each month, after we verified the data, beer sellers received a maximum wage of $30. Beer sellers were repeatedly informed that we were doing our best using statistical techniques to detect whether the data were real.

The smartphone application contained the following information. First, the app requested, for each variety, the prices and quantities of bottles sold on that day. Second, again for each variety, the beer seller reported the prices and quantities of bottles purchased that day. Third, the beer seller inspected his inventory and reported the number of bottles in stock each day. Fourth, the beer seller recorded taxes paid, the occurrences of attacks, thefts, the number of beers sold on credit, the running unpaid credits from sales, as well as other types of purchases and investments made on the store.

Finally, using a similar smartphone application, a second individual in the village, the "intellectual", recorded contextual information about the village, in particular violent events and theft.

Of the 45 retailers involved in the data collection, 41 initially agreed to participate in the study. The four who did not participate did so because of their religious beliefs. Of the 41 beers sellers, one beer seller changed his mind a few days after the date of his ritual was assigned. Further, five retailers did not submit consistent or reliable data during the full period of the analysis. Thus, removing these six retailers leaves us with a sample of 35 beer retailers attending the rituals and for which we have data for the entire study period. Summary statistics for the sample are reported in Table C.1.

3.5 Empirical strategy

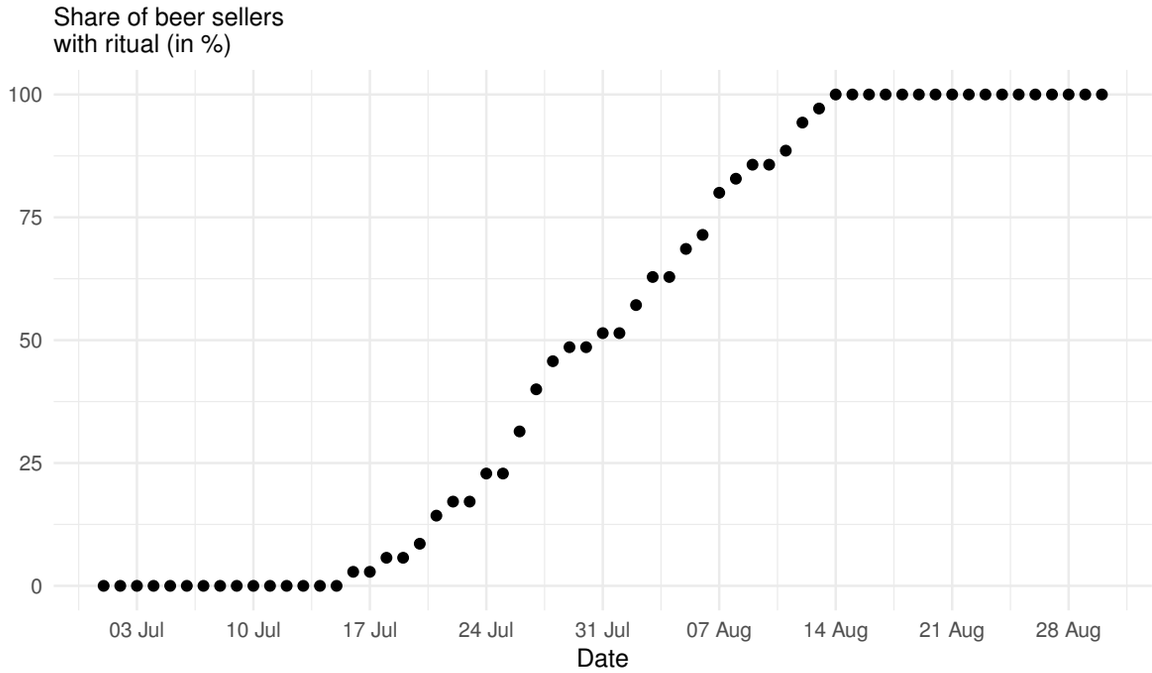
We now turn to a description of our experimental design and our estimating equations.

3.5.1 Assignment to the intervention

Our study design is one where all participants receive treatment, but the order in which this is done is randomized. Our sample of 35 beer sellers was treated over a period of approximately thirty days from July 17th to August 13th 2017. Treatment occurred in cities in the three regions represented in our sample.[5] Each beer seller was given transport to the location of the

[5]The cities were Masisi Centre Kibabi, and Rubaya Centre in Masisi; Vitchumbi, Kanyabayonga, Rubare, Kibirzi, and Kiwanja in Rutshuru; and Itebero, Makana, Ndjingala, and Walikale Centre in Walikale.

Figure 3.2: Share of beer sellers that had received treatment by day.



ritual the day before the ritual. The following day he received the ritual and then traveled back to his village on the same or the following day. In each day, one beer seller was assigned to receive treatment by the witchdoctor. Doing the rituals more frequently was not feasible since it would have been too taxing on the witch doctor's magic.

There was one exception to this rule when, related to the violations of treatment assignments, one of the witchdoctors had to perform three rituals.

Figure 3.2 reports the fraction of all beer sellers that were treated in each day. As reported, the assignment to treatment occurred smoothly during a one-month period. Treatment generally occurred as planned. However, given the nature of the setting of the study and the resulting logistical difficulties, there were some instances in which treatment did not occur during the assigned treatment date. Violence and security concerns forced enumerators to deviate from the randomized treatment assignment in some cases by a day or so – a full list of such cases is documented in the paper's appendix.

3.5.2 Estimating Equations

3.5.2.1 Waitlist Design

The set-up of the experiment, where all participants eventually receive treatment but at different randomly-determined times, is commonly referred to as a waitlist design. It is common in the medical field, where having a control group for which treatment is withheld is not possible for ethical reasons. In this design, the probability of assignment to treatment varies from day to day. The probability that an untreated retailer receives treatment is much higher during the last days of the experiment than during the first days. Thus, to obtain unbiased estimates of the average treatment effects, treated observations are weighted by the inverse probability of

being assigned to the treatment group, and control observations are weighted by the inverse probability of being assigned to the control group.

The equation that we estimate is given by:

$$Y_{it} = \alpha_i + \delta_{w(t)} + \beta D_{it} + \varepsilon_{it}, \tag{3.1}$$

where i denotes beer retailers, t denotes days from July 17th to August 13th, 2017, which is the period during which treatment was occurring; during this time, there is always one beer seller in the treatment group and one in the control group. Y_{it} is an outcome of interest, D_{it} is an indicator that equals one when beer seller i receives the ritual.[6] α_i denotes beer-retailer fixed effects, which account for average differences across beer sellers. $\delta_{w(t)}$ denotes day-of-week fixed effects and week-of-year fixed effects. The former capture average differences within a week. For example, alcohol consumption may be higher on Sundays, a day when less work is done. The latter capture average differences between weeks. For example, alcohol consumption may be lower during the dry season.

3.5.2.2 Estimation Using Alternative Definitions of Treatment

As we have noted, in some cases, the actual day of treatment deviated from the randomized day of treatment, due to delays in implementation that arose due to logistical and security issues. Given this, we also report estimates where instead of using D_{it} as our treatment measure, we use a treatment measure that is based on assignment to treatment (and not actual receipt of treatment). We refer to this as our ITT estimate. We denote this variable T_{it}. We also report estimates where we use treatment assignment T_{it} as an instrument for actual treatment D_{it}.

3.5.2.3 Allowing for a Differential Effect Following a Post-Ritual Theft

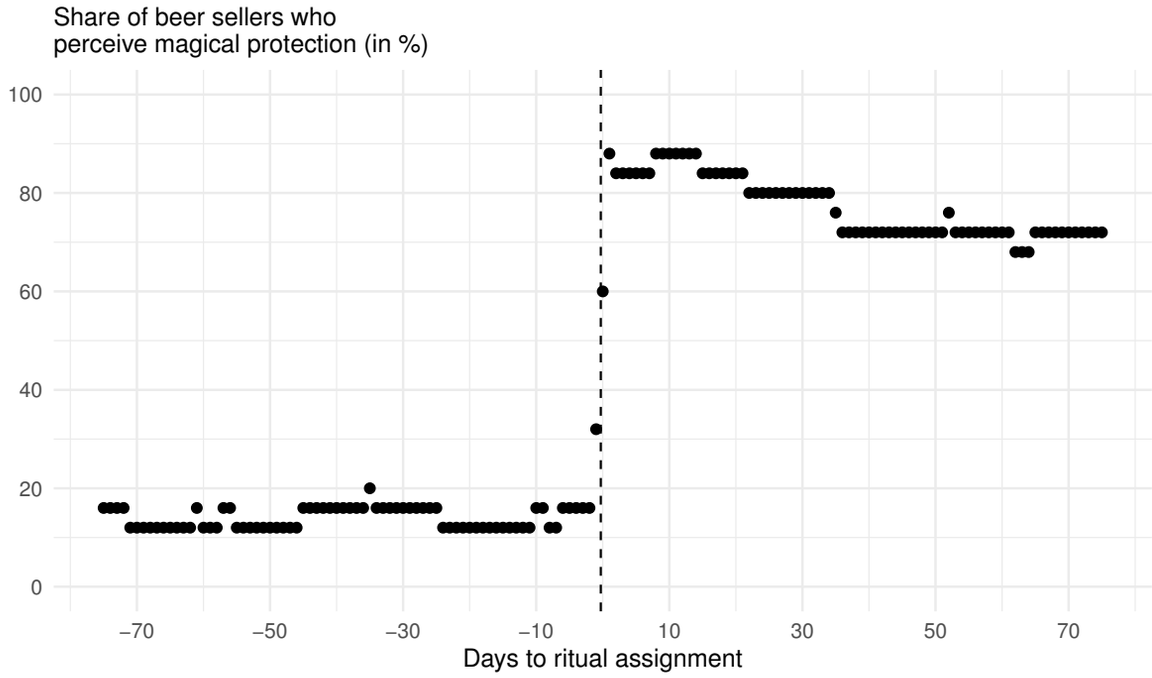
It is possible that the effects of the ritual on our outcomes of interest are very different if a retailer is robbed. If a theft occurs, this will likely cause the retailer to question the validity of the perceived protection, weakening its effects. We test for this by adding the following additional interaction term to our estimating equations: $D_{it} \times PostTheft_{it}$, where D_{it} is the post-ritual indicator and $PostTheft_{it}$ is an indicator variable that equals one following a theft. The interaction between the two is equivalent to an indicator variable that equals one following a post-ritual theft.

3.5.2.4 Estimation of an Effect on Sale Prices

Any effect on revenues may be due to a change in quantities sold or in sale prices. The data set contains several brands of beer and other beverages. Some of the brands are typically more expensive than others. To test for effects on sale prices in a single regression, it is thus necessary to modify the data set and the estimating equation. We expand the data set such that an observation is a beer brand by beer seller by day, and we add beer brand fixed effects to our estimating equation.

[6] Results are unchanged when we drop the day on which the ritual took place from the analysis.

Figure 3.3: Effect of treatment on perceptions of protection



3.6 Results

We now turn to our estimated effects of the ritual treatment on the behavior of beer sellers. The first outcome that we examine is whether beer sellers perceived that they were protected by a ritual. This step can be viewed as a zero-stage estimate or a check on the validity of our treatment.

There are many reasons that people may not believe they are protected despite undergoing a protection ritual. For example, they may not believe that the witchdoctor was legitimate. Alternatively, they could believe in the ritual but were not able to follow the conditions necessary for it to remain in effect.

Given our interest in the effects of the ritual on those who believe in witchcraft, we partition our sample into two groups: believers and non-believers, as revealed by the data. Specifically, we define a beer-seller as non-believer if they report never being protected even after receiving the protection ritual. In total, this is true for 10 of the 35 beer sellers. Thus, 25 beer sellers report being protected for at least one day in the sample period.

The effectiveness of the ritual for the sample of 25 beer sellers is summarized in Figure 3.3, which reports the fraction of beer sellers that report having some form of magical protection in use in each day. The days are measured as days before actual treatment and days after treatment.[7] A number of facts are apparent from Figure 3.3. First, prior to treatment, some beer sellers (about 15% on average) did have their own magical protection. Second, there is a noticeable increase in the proportion of beer sellers that were protected following treatment. Third, there is some evidence that, following the ritual, the perceived protection wears off. One

[7]The same figure but using the assignment to treatment rather than actual treatment is reported in Appendix Figure C.1.

reason why this could occur, and one that we will test for explicitly, is that if theft occurs following a ritual, then a retailer may question the validity of the ritual, perhaps believing that the spell has worn off because they have not correctly followed the conditions. Thus, the effects of a ritual may be very different if theft occurs. We return to this below.

3.6.1 Effect of the ritual on business outcomes

We begin by first reporting our waitlist design estimates. Panel A of Table 3.1 reports our baseline ITT estimates. We report coefficients, standardized 'beta' coefficients in italics, and p-values from randomization inference in square brackets. The odd-numbered columns report estimates with without day-of-week and week-of-year fixed effects and the even-numbered columns with day-of-week and week-of-year fixed effects.

In columns 1 and 2, we report estimates of the effects of the ritual on daily revenues. We find a large and statistically significant positive effect. Beer sellers make roughly $3 more revenue each day on average after the ritual. We disaggregate this estimated revenue effect into prices and quantities by separately examining the number of bottles sold (columns 3 and 4), and the average unit price of bottles sold (columns 5 and 6). We find that the increase in revenues is due to increased sales and not prices. This is reassuring since market prices fluctuate very little and that the vendors tend to be price takers. If we saw large effects working through prices, we might be worried that optimistic reporting, influenced by the ritual treatment, may be driving some of the results.

In Panel B of Table 3.1, we report estimates that allow for a differential effect of treatment in the days that follow a robbery. If a beer seller, who perceives himself to be protected, is stolen from, this may cause him to question the protection. Alternatively, he may believe that this is proof that he has not followed the necessary conditions correctly. Thus, Panel B in Table 3.1 reports estimates identical to Panel A except that we allow the estimated effect of the ritual to differ in the days following a theft. The effects that we estimate for revenues and quantities are entirely reversed (and even more so) following a theft. The effect of rituals following a post-ritual theft can be obtained by summing the two estimated effects.[8]

While the beer-sellers' primary sales item is beer, they also stock and sell local soft drinks. The estimates reported to this point have included all beverages together. We now check the robustness of our estimates to distinguishing between beer and soft drinks. Ex ante, we do not have any reason to expect that one would be affected more than another. Both beverages are valuable and can be stolen. The estimates, which are reported in Table C.3, show that we obtain very similar estimates for soft drinks and for beer. The magnitude of the estimated effects is similar for the number of bottles sold, while the revenue effects are larger for beer, which is expected given that the price per bottle is higher (roughly $1.80 versus $0.90)

To assess whether the ritual is a profitable investment, we compare the extra profit beer sellers make from increased sales to the cost of the ritual. Within the period we study in the waitlist design, beer sellers make, on average, roughly $1 more profit each day after the ritual ($3 from increased sales minus $2 cost of goods sold). Thus, the average increase in profits is $14

[8]We find the same result when including theft incidences that took place one or two weeks after the period we study in the waitlist design: a theft exactly reverses the effects of the ritual.

Table 3.1: Main results: intent-to-treat and IV analyses of business outcomes

	Revenues (USD)		Number of bottles sold		Sale Prices (USD)	
	(1)	(2)	(3)	(4)	(5)	(6)
			Panel A. Baseline			
Intent-to-treat	3.018	3.829	1.899	2.809	0.025	−0.012
Beta coefficient	0.069	0.088	0.049	0.073	0.022	−0.011
IV	3.213	4.253	2.021	3.120	0.029	−0.016
p value for no effect	*0.002*	*0.001*	*0.003*	*0.003*	*0.936*	*0.941*
			Panel B. Post-Treatment Theft			
Intent-to-treat	3.556	3.597	2.433	2.585	0.019	−0.009
Beta coefficient	0.081	0.082	0.063	0.067	0.016	−0.007
IV	3.793	3.997	2.596	2.872	0.021	−0.011
p value for no effect	*0.003*	*0.005*	*0.005*	*0.007*	*0.902*	*0.895*
Post-treatment theft (ITT)	−6.455	−7.029	−6.412	−6.809	0.066	0.052
Beta coefficient	−0.097	−0.105	−0.109	−0.115	0.037	0.029
Post-treatment theft (IV)	−6.539	−6.976	−6.470	−6.771	0.066	0.051
p value for no effect	*0.133*	*0.129*	*0.106*	*0.084*	*0.222*	*0.138*
Beer seller fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Beer brand fixed effects	No	No	No	No	Yes	Yes
Week-of-year fixed effects	No	Yes	No	Yes	No	Yes
Day-of-week fixed effects	No	Yes	No	Yes	No	Yes
Mean (std. dev.) dep. var.	14.34 (21.88)		11.82 (19.37)		1.23 (0.59)	
Observations	645	645	645	645	3,157	3,157

Notes: This table presents intent-to-treat and IV estimates of the effects of the ritual on business outcomes. The dependent variable in columns 1 and 2 is revenues (converted to USD); in columns 3 and 4, it is number of bottles sold; in columns 5 and 6, it is sale prices (converted to USD). In columns 1 to 4, the unit of observation is beer seller by day; in columns 5 and 6, it is beer brand by beer seller by day. All regressions include beer seller fixed effects. Columns 2, 4, and 6 add day-of-week and week-of-year fixed effects; and columns 5 and 6 also include brand fixed effects. For each regression, intent-to-treat coefficients, beta coefficients of the ITT, IV coefficients, and p-values for no effect (in italics) are reported. p-values are computed using randomization inference with 10,000 reassignments of treatment status. All regressions are weighted by the inverse probability of being treated.

within these two weeks. Looking beyond the period of the waitlist design, the average increase in profits is $40 after one month and $61 after two months. On the other hand, the market price for a ritual is around $50. Thus, our findings suggest that investment in a ritual roughly breaks even.

3.6.2 Understanding Mechanisms

Our findings of increased sales and revenues raise the natural question of why. To dig deeper into the reason behind this effect, we next estimate the effects of the protection rituals on mechanisms that could explain the increase in sales and revenues. Specifically, we test whether the rituals affected purchases of new bottles and the incidence of stock-outs, defined as zero total inventories at any point in a day. If the ritual affected the beer seller's perceived probability of being robbed, this may have affected the stock of inventories that sellers felt was safe to hold, which, in turn, could affect the frequency of stock-outs.

The estimated effects of the protection ritual on the two outcomes are reported in columns 1–4 of Table **??**. We find a sizeable positive effect of the ritual on purchases of new bottles and a negative effect on stock-outs. While the magnitudes for both sets of estimates is large, the precision of the estimates for purchases is much greater than for stock-outs. This is likely due to the infrequency of stock-outs in our sample. There is only a 5.43% chance that a beer seller will experience a stock-during during a day. The coefficients estimated for the stock-out effects are large; they imply that the ritual reduced the occurrence of stock-outs by about 5 percentage points. This is a very large effect, especially when compared to the mean occurrence of stock-outs in the sample, which is 5.43%.

We also allow these estimated effects to differ following a post-ritual theft. These estimates are reported in columns 1–4 of Table 3.2 Panel B. As with sales and revenues, we see that the effects of the ritual are completely reversed if theft occurs following a ritual.

The last outcome that we examine is the closing time, measured as a variable that ranges from 4 (4am) to 27 (3am). A priori, it is possible that due to the ritual, sellers feel that they can leave their shops open later. If so, this would also potentially account for the greater sales. Another possibility is that beer sellers engage in daily income targets and close their shop if such a target is reached. Such behavior is common in the DRC. For example, this is a well-known strategy practice by drivers of motorcycle taxis. If this is the case, then the increased sales and revenues due to the protection spell may cause sellers to close earlier.

The estimates of the effect of the ritual on shop closing times are reported in columns 5 and 6 of Table 3.2. Interestingly, we find evidence that the ritual causes beer sellers to close their shops earlier, which is consistent with daily income targeting. The estimated coefficients, although imprecisely estimated, are negative. The estimates allowing for a differential effect of following a post-ritual theft are reported in columns 5 and 6 of Table 3.2 Panel B. Interestingly, we do not see the same reverse of the hours-opening effect following a post-ritual theft. In fact, we estimate an intensification effect following a theft after a ritual that is statistically significant. That is, following a post-ritual theft, beer sellers tend to close their shop even earlier. However, one caveat for all of these estimates is that they are small in magnitude. The estimated effects range from 6–15 minutes.

Table 3.2: Main results: intent-to-treat and IV analyses of purchases, stock-outs, and closing times

	Number of bottles purchased		Stock-out indicator		Closing time	
	(1)	(2)	(3)	(4)	(5)	(6)
			Panel A. Baseline			
Intent-to-treat	3.891	11.282	−0.051	−0.037	−0.156	−0.087
Beta coefficient	0.023	0.068	−0.112	−0.081	−0.057	−0.031
IV	4.143	12.531	−0.054	−0.041	−0.167	−0.097
p value for no effect	*0.023*	*0.044*	*0.288*	*0.353*	*0.160*	*0.104*
			Panel B. Post-Treatment Theft			
Intent-to-treat	4.446	11.115	−0.058	−0.033	−0.138	−0.092
Beta coefficient	0.027	0.067	−0.129	−0.073	−0.050	−0.033
IV	4.743	12.351	−0.062	−0.037	−0.147	−0.102
p value for no effect	*0.023*	*0.046*	*0.224*	*0.384*	*0.376*	*0.091*
Post-treatment theft (ITT)	−6.650	−5.072	0.091	0.117	−0.224	−0.140
Beta coefficient	−0.026	−0.020	0.132	0.169	−0.053	−0.033
Post-treatment theft (IV)	−6.755	−4.909	0.093	0.116	−0.221	−0.141
p value for no effect	*0.038*	*0.026*	*0.000*	*0.000*	*0.000*	*0.000*
Beer seller fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Week-of-year fixed effects	No	Yes	No	Yes	No	Yes
Day-of-week fixed effects	No	Yes	No	Yes	No	Yes
Mean (std. dev.) dep. var.	15.76 (82.98)		0.05 (0.23)		20.28 (1.38)	
Observations	645	645	645	645	639	639

Notes: This table presents intent-to-treat and IV estimates of the effects of the ritual on purchases, stock-outs and closing times. The unit of observation is beer seller by day. The dependent variable in columns 1 and 2 is number of bottles purchased; in columns 3 and 4, it is an indicator variable that equals one if beer sellers stock was zero; in columns 5 and 6, it is the time when beer sellers closed the shop. All regressions include beer seller fixed effects. Columns 2, 4, and 6 add day-of-week and week-of-year fixed effects. For each regression, intent-to-treat coefficients, beta coefficients of the ITT, IV coefficients, and p-values for no effect (in italics) are reported. p-values are computed using randomization inference with 10,000 reassignments of treatment status. All regressions are weighted by the inverse probability of being treated.

We next turn to the question of what mechanisms explain higher purchases and lower stock-outs. We do this by studying the potential effects of the ritual on deeper mechanisms like the beliefs, expectations, and psychology of beer sellers. The first that we consider are people's beliefs about the extent to which they are protected. This is measured using a question that asks: "*Do you have any fetish in use today?*" and they choose "yes" or "no". The estimates, reported in columns 1 and 2 of Table 3.3 show that, indeed, people do feel more protected following the ritual and that this feeling is partially reversed following a theft. Next, we look more specifically at their perceived expectation that they will be robbed. This is measured using a question that asks: "*Do you think someone will steal from you next week*" and they choose "yes" or "no". Estimates with an indicator that equals one if the retailer answered "yes" are reported in columns 3 and 4. Consistent with beer sellers feeling more protected, we find that the ritual lowers their expectation of being robbed. The effect is sizable – reducing theft expectations by about 9 percentage points on average – and the estimated effects is statistically significant at conventional levels. Interestingly, we find no evidence that this belief is reversed following a post-ritual robbery (Panel B). In columns 5 and 6, for completeness, we also report the estimated effects of the ritual on actual theft. This provides a placebo test (assuming the ritual does not really work). It is also useful as evidence for whether there are experimenter demand effects, which generate reporting bias. If so, we might expect individuals to be less likely to report actual thefts that occur so as not to disappoint us. Reassuringly, we find no evidence that the ritual affects the actual incidence of theft.

We also examine the effects of the ritual on a self-reported measure of stress. Respondents are asked "*What is your stress level today*". They then rate their stress level from 0 (no stress) to 10 (high stress). Estimates with this outcome are reported in columns 7 and 8 of Table 3.3. In contrast to previous findings in the literature, we find no evidence that the rituals helped to reduce self-reported stress. The estimates are highly insignificant and very small in magnitude. This is potentially explained by the fact that the reported values of stress are quite low. The mean value reported is only 1.42.

3.6.3 Non compliers as a control

For completeness, and as a check on the sensibility of our estimates, we also report the same regression estimates, but for the ten retailers who we infer do not believe in traditional magic. These are those who never report (neither before nor after treatment) being protected by magic. The estimates, which are reported in Appendix Tables C.4–C.6, show that we do not find the same effect of treatment as we do with the other retailers. Specifically, we find no effects on sales revenues, quantities sold, inventories, stock-outs, or expectations of theft.

Table 3.3: Main results: intent-to-treat and IV analyses of perceptions, expectations, and emotions

	Feeling protected indicator		Theft expectation indicator		Theft indicator		Stress level [0-10]	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
				Panel A. Baseline				
Intent-to-treat	0.687	0.651	−0.087	−0.077	−0.001	0.005	0.050	0.065
Beta coefficient	0.693	0.657	−0.127	−0.112	−0.004	0.019	0.010	0.013
IV	0.731	0.723	−0.093	−0.085	−0.001	0.006	0.053	0.072
p value for no effect	*0.000*	*0.000*	*0.024*	*0.043*	*0.650*	*0.342*	*0.267*	*0.304*
				Panel B. Post-Treatment Theft				
Intent-to-treat	0.707	0.641	−0.079	−0.080			0.036	0.074
Beta coefficient	0.713	0.647	−0.116	−0.117			0.007	0.015
IV	0.754	0.713	−0.084	−0.089			0.038	0.082
p value for no effect	*0.000*	*0.000*	*0.105*	*0.034*			*0.198*	*0.280*
Post-treatment theft (ITT)	−0.237	−0.294	−0.095	−0.094			0.165	0.269
Beta coefficient	−0.157	−0.195	−0.091	−0.091			0.022	0.036
Post-treatment theft (IV)	−0.254	−0.285	−0.093	−0.096			0.164	0.271
p value for no effect	*0.001*	*0.004*	*0.000*	*0.000*			*0.996*	*0.966*
Beer seller fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Week-of-year fixed effects	No	Yes	No	Yes	No	Yes	No	Yes
Day-of-week fixed effects	No	Yes	No	Yes	No	Yes	No	Yes
Mean (std. dev.) dep. var.	0.43 (0.50)		0.13 (0.34)		0.02 (0.14)		1.42 (2.47)	
Observations	645	645	645	645	645	645	645	645

Notes: This table presents intent-to-treat and IV estimates of the effects of the ritual on perceptions, theft expectations, actual theft, and perceived stress. The unit of observation is beer seller by day. The dependent variable in columns 1 and 2 is an indicator variable equal to one if beer seller feels protected from theft; in columns 3 and 4, it is an indicator variable equal to one if beer seller expects a theft incidence in the following week; in columns 5 and 6, it is an indicator variable equal to one if retailer experienced a theft incidence during the day; and in columns 7 and 8, it is self-reported stress level. All regressions include beer seller fixed effects. Columns 2, 4, 6, and 8 add day-of-week and week-of-year fixed effects. For each regression, intent-to-treat coefficients, beta coefficients of the ITT, IV coefficients, and p-values for no effect (in italics) are reported. p-values are computed using randomization inference with 10,000 reassignments of treatment status. All regressions are weighted by the inverse probability of being treated.

3.7 Conclusion

Traditional religious beliefs, such beliefs in ancestors, supernatural powers, and witchcraft, appear to be widespread in sub-Saharan Africa. Within anthropology, they are well-recognized and widely studied. They are described as permeating all aspects of daily life. Survey data indicate that the majority of the population of sub-Saharan Africa hold these traditional beliefs. Despite this, within economics, these beliefs have not been incorporated into our understanding of human behavior, whether it be in the social or economic sphere. One reason for this is that from the existing evidence, it is far from clear how strongly held these beliefs are and whether they actually affect human actions, particularly in the economic sphere.

In this study, we have made progress on this issue by estimating the effects of a common anti-theft ritual among a random sample of beer retailers in the Eastern DRC. The study provided a protection ritual to the retailers at different points in time that are randomly chosen. Observing daily economic behavior and reported beliefs before and after participation in the ritual, we found that ten of the 35 retailers did not believe in the efficacy of the ritual they undertook. This proportion is roughly consistent with the prevalence of witchcraft beliefs in the region 70-80%.

We found that, among believers, participation in the ritual had noticeable effects on beliefs and economic behavior. In the weeks following the ritual, the retailers believed that the ritual was working, perceived a lower probability of theft, held larger stocks of inventory, had fewer stock-outs, and had higher revenues and profits. We did not observe the effects among the retailers who report not believing in the efficacy fo the ritual.

Thus, our findings showed that these beliefs are strongly held and enough so to affect economic behavior within our sample of retailers. This insight is particularly important given the reported prevalence of traditional religious beliefs and their persistence, in spite of the widespread adoption of Christianity and Islam on the continent. Such beliefs are real and do affect decisions and outcomes that are relevant for economic development. The findings here point to the importance of additional research aimed at understanding these beliefs and their consequences within developing-country contexts.

Part III

Appendices

A Appendix to Chapter 1

Figure A.1: Country-level drought intensity exposure among WVS countries over years

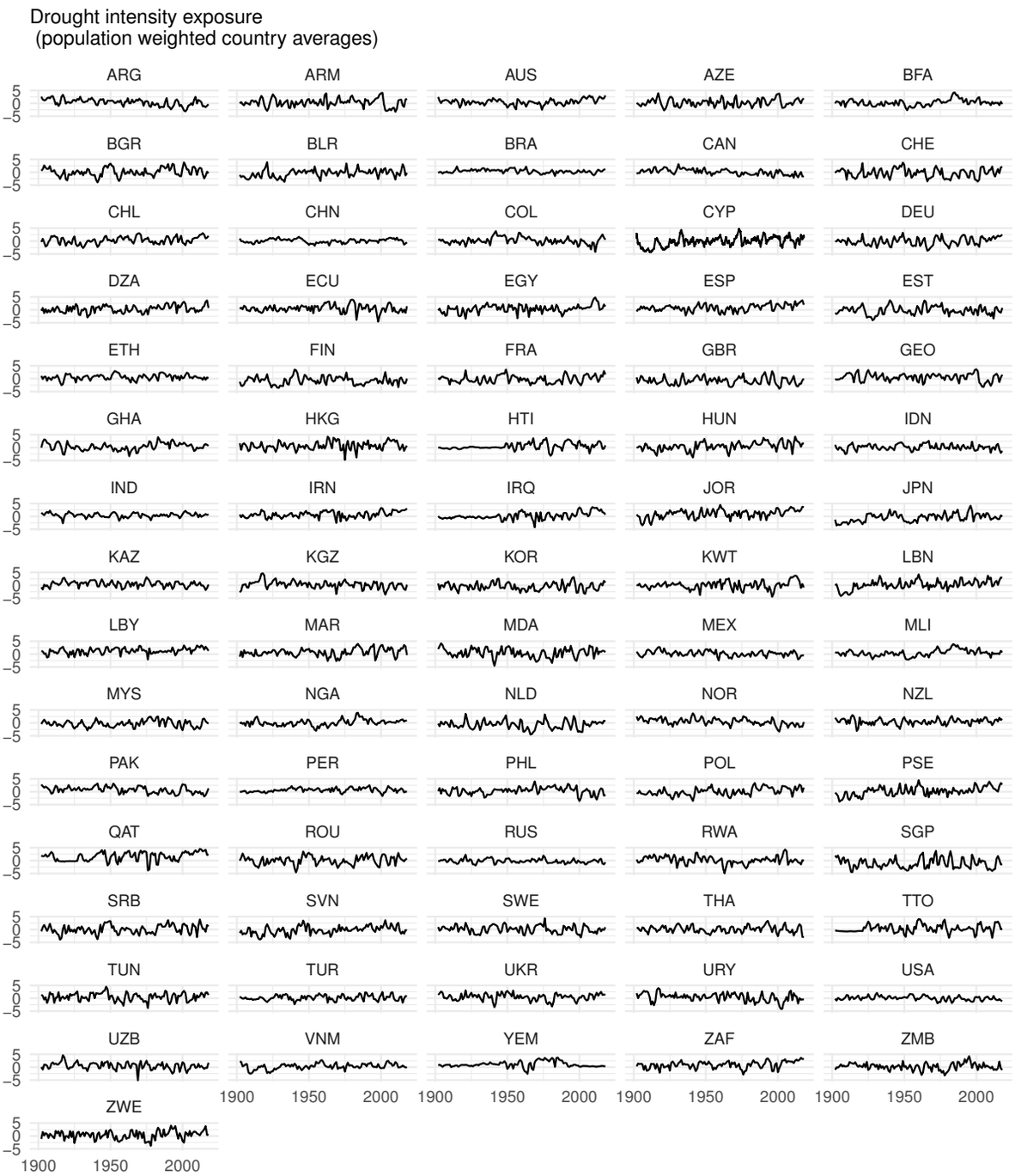


Figure A.2: Country-level earthquake exposure among WVS countries over years

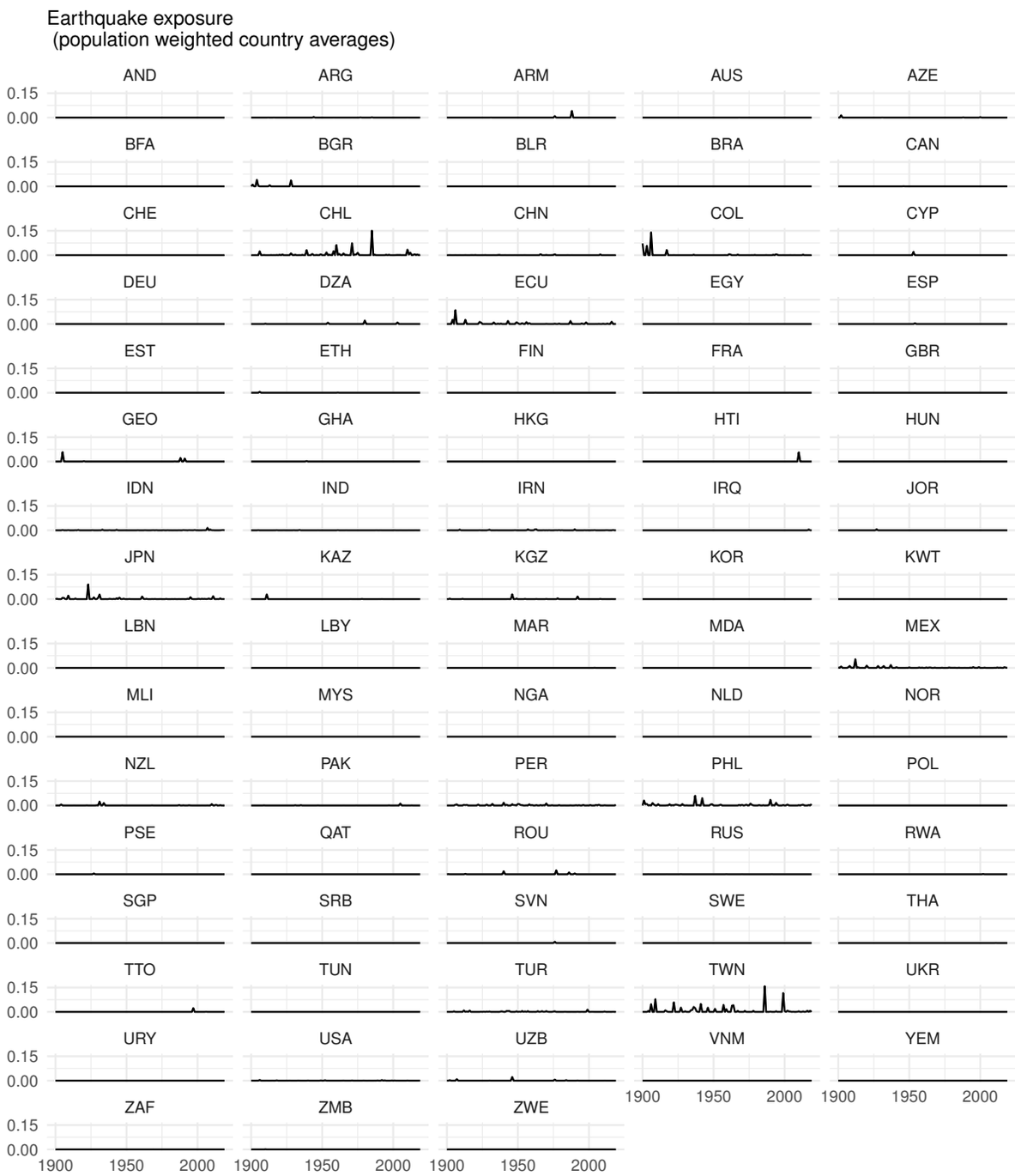


Figure A.3: Country-level hurricane exposure among WVS countries over years

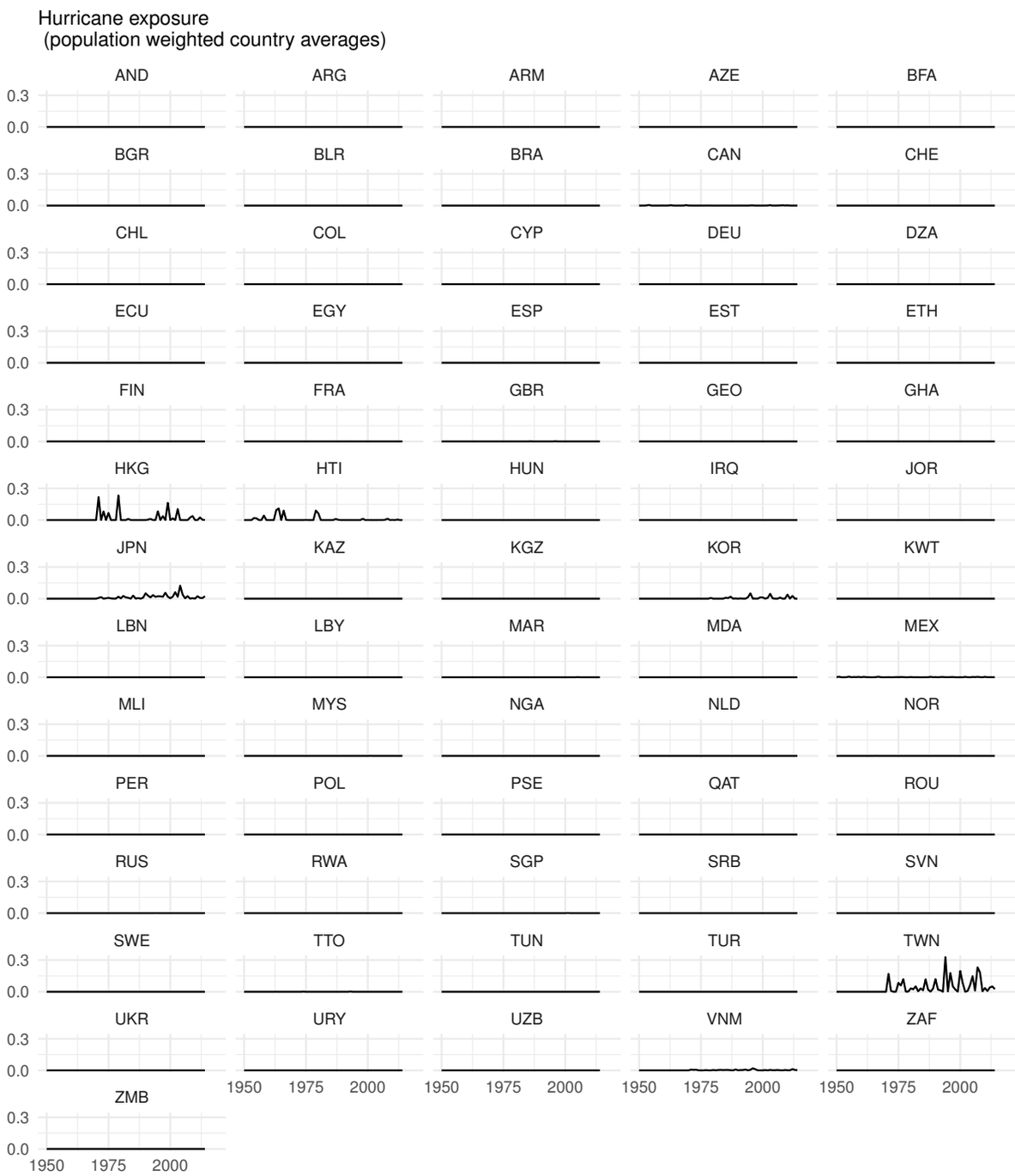


Figure A.4: Economic disaster exposure among WVS countries over years

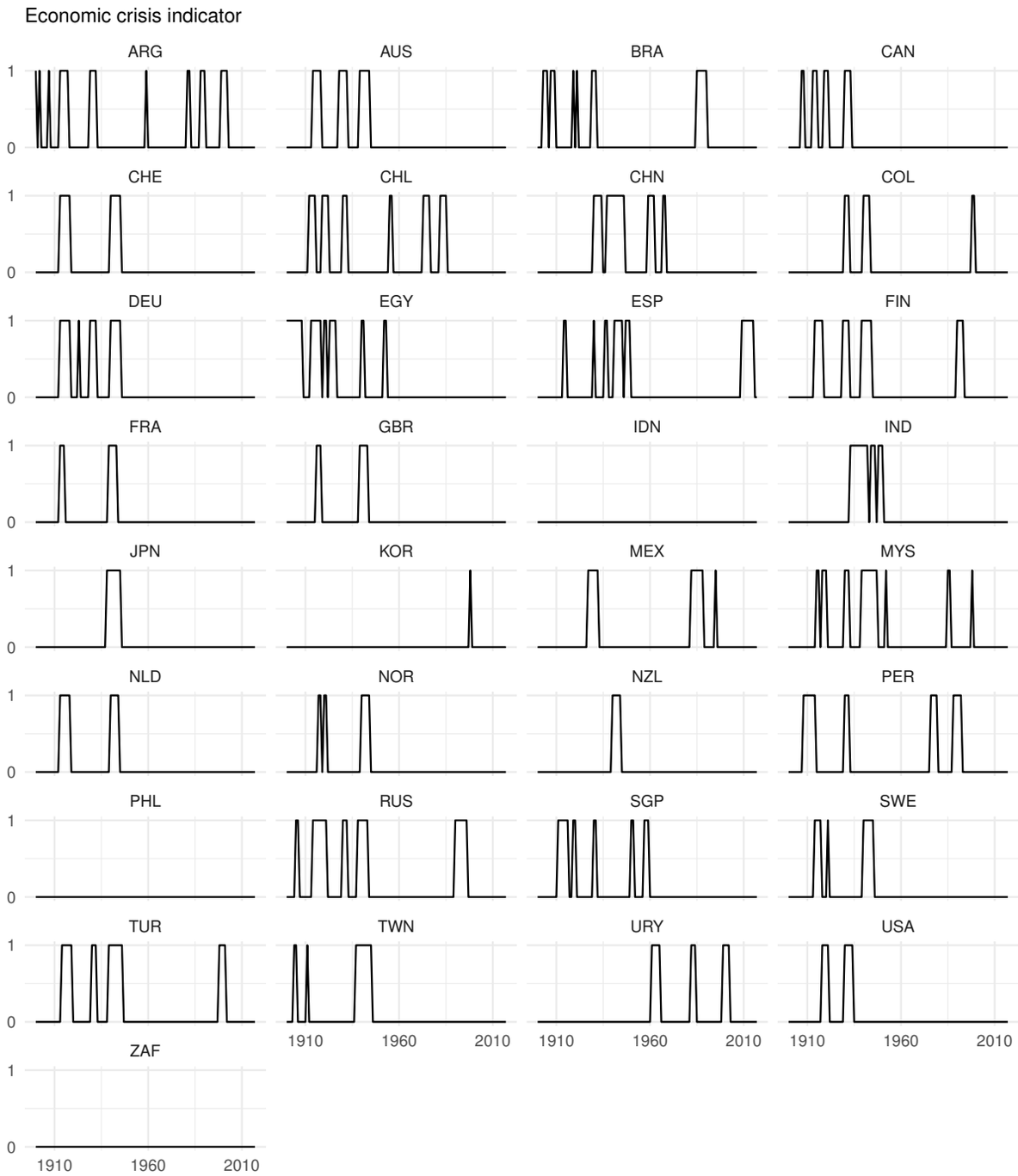


Figure A.5: Country-level average drought experience across countries covered by the WVS

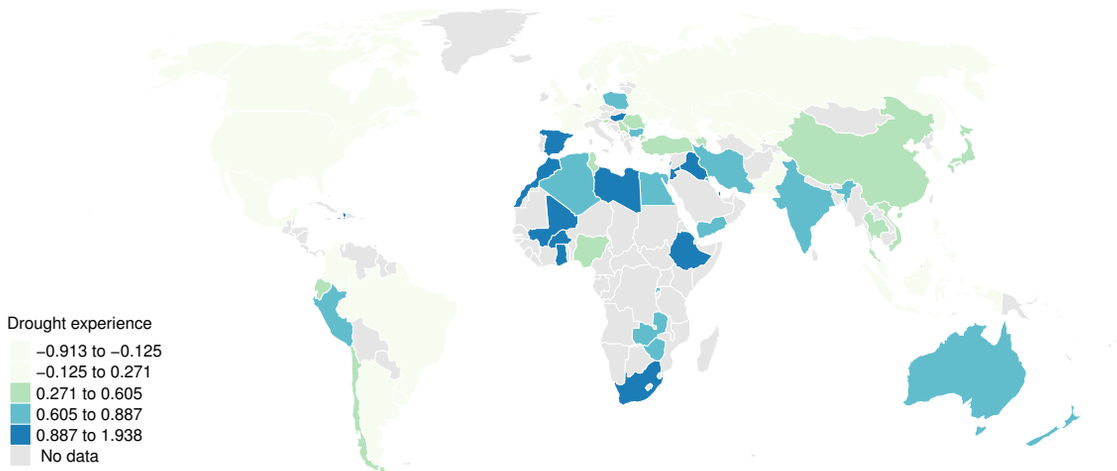


Figure A.6: Country-level average earthquake experience across countries covered by the WVS

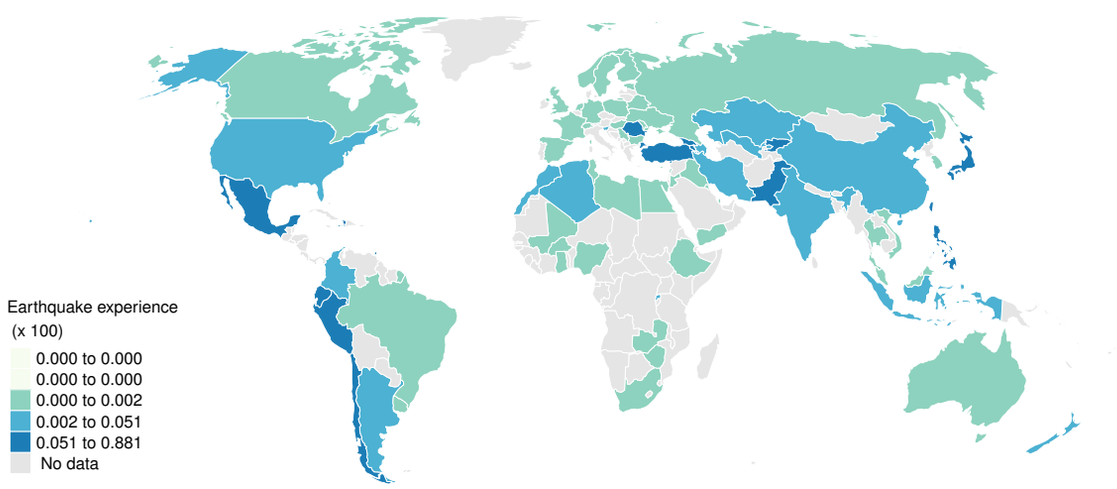


Figure A.7: Country-level average hurricane experience across countries covered by the WVS

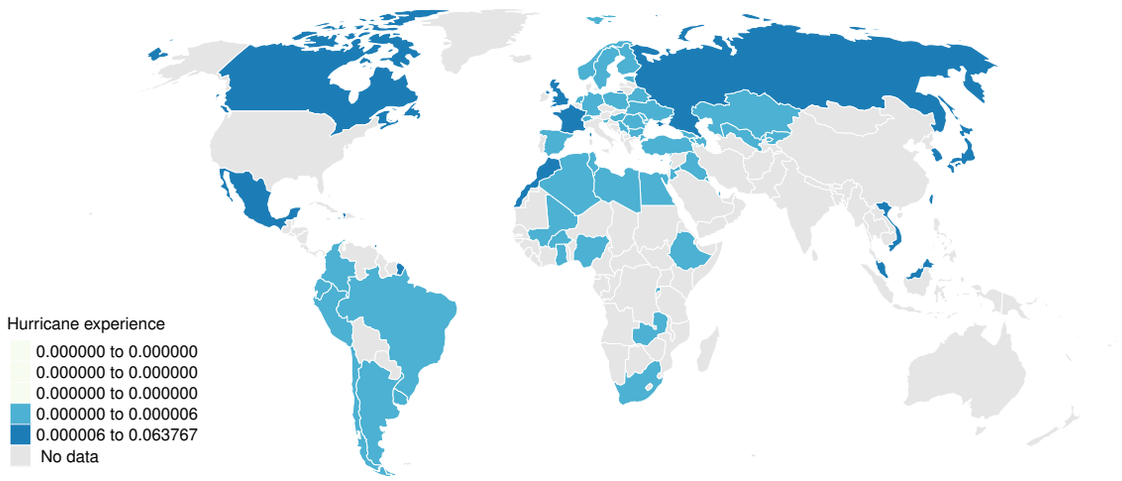


Figure A.8: Country-level average economic disaster experience across countries covered by the WVS

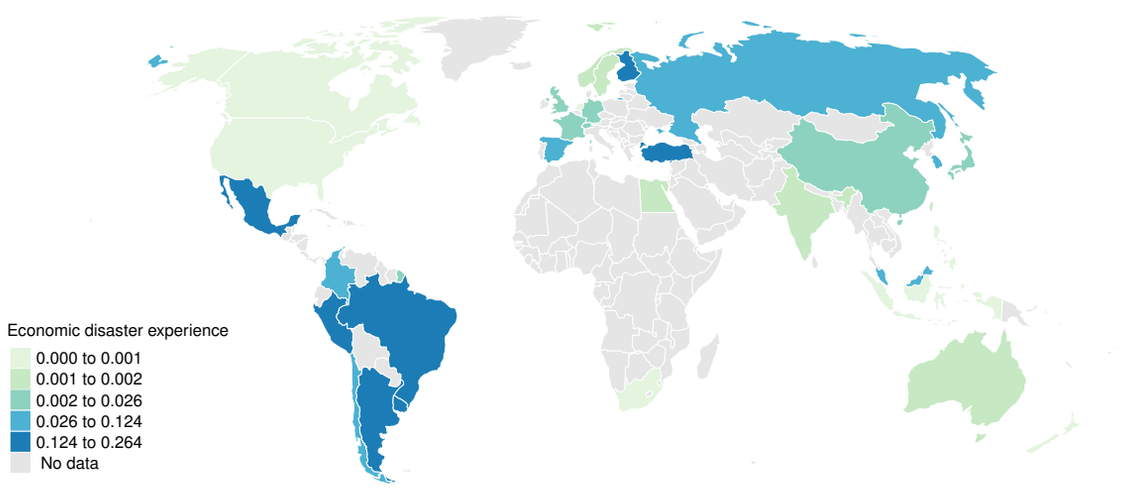


Table A.1: Robustness: restricting sample to native-born respondents

	Importance of norm conformity [std.]			
	(1)	(2)	(3)	(4)
Drought experience [std.]	−0.004			
	(0.032)			
Drought experience × High agriculture [std.]	0.032			
	(0.025)			
Earthquake experience [std.]		0.021		
		(0.035)		
Hurricane experience [std.]			0.053	
			(0.042)	
Economic disaster experience [std.]				0.074***
				(0.024)
No. of countries	52	53	45	22
Country × survey-year FE	Yes	Yes	Yes	Yes
Cohort, age, gender FE	Yes	Yes	Yes	Yes
N	57,258	58,186	42,120	27,561
R^2	0.090	0.090	0.095	0.078

Notes: OLS estimates, robust standard errors are clustered at the country-cohort level and reported in parentheses. An observation is an individual. The outcome in all columns is self-reported importance of norm conformity. The main independent variables are lifetime experience of drought intensity interacted with an indicator equal to one if the average share of employment in agriculture in years 2000 - 2005 is greater than 25%, lifetime experience of significant earthquakes, lifetime experience of hurricanes, and lifetime experience of economic disasters. The data sources of the natural and economic disasters and the measurement of lifetime experience are described in the text. All regressions include fixed effects for country of current residence × survey year, birth cohort, age, and gender. Other individual controls include a dummy equal to one if the respondent is unemployed and fixed effects for highest educational attainment. All variables are standardized to z-scores. *** $p < 0.01$, ** $p < 0.05$, and * $p < 0.1$.

Figure A.9: Robustness of drought results to dropping one country or cohort at a time.

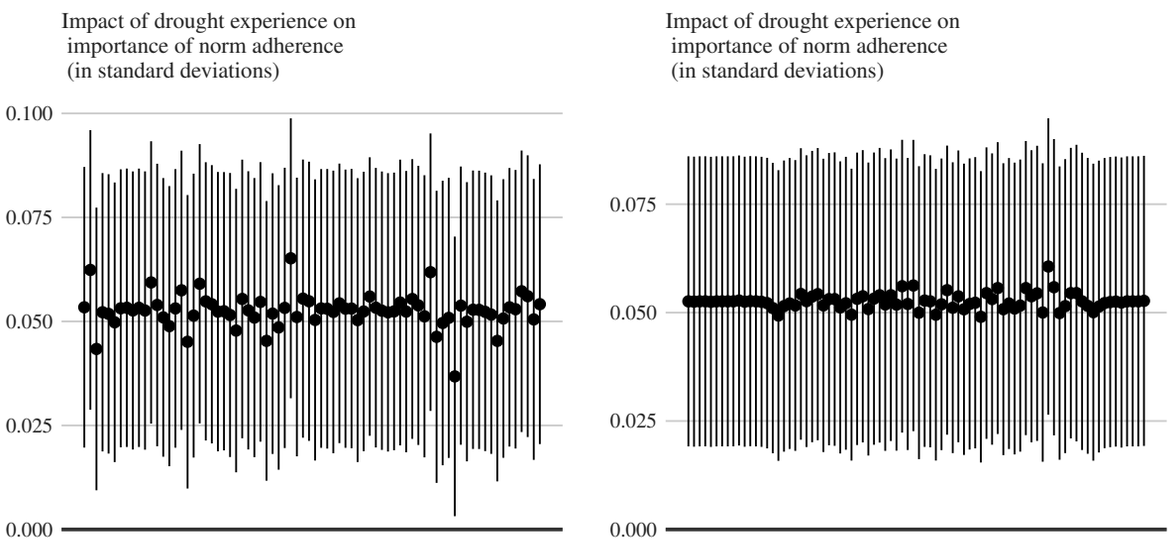


Figure A.10: Robustness of earthquake results to dropping one country or cohort at a time.

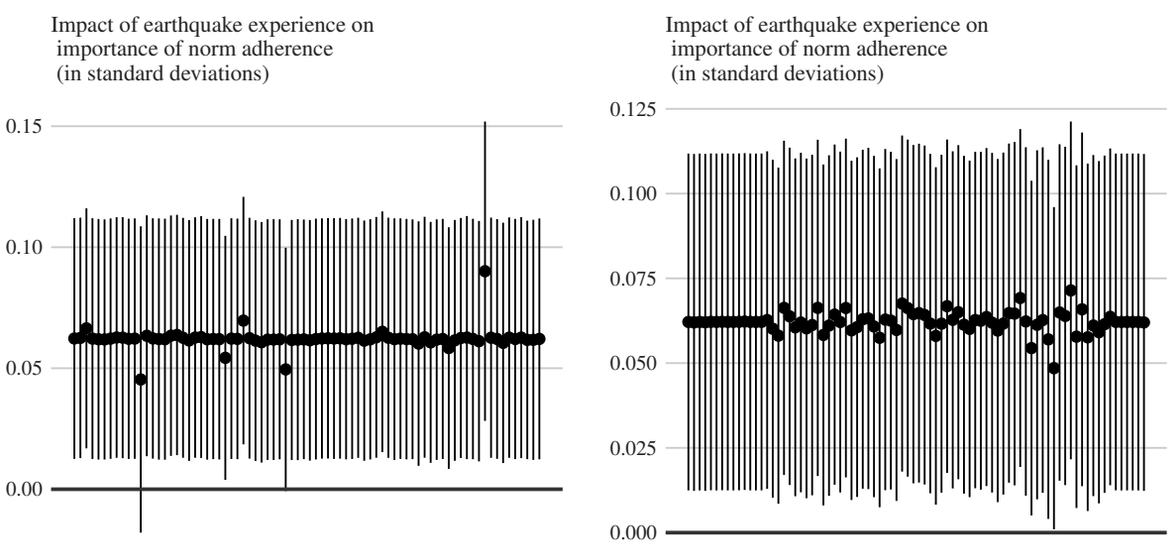


Figure A.11: Robustness of hurricane results to dropping one country or cohort at a time.

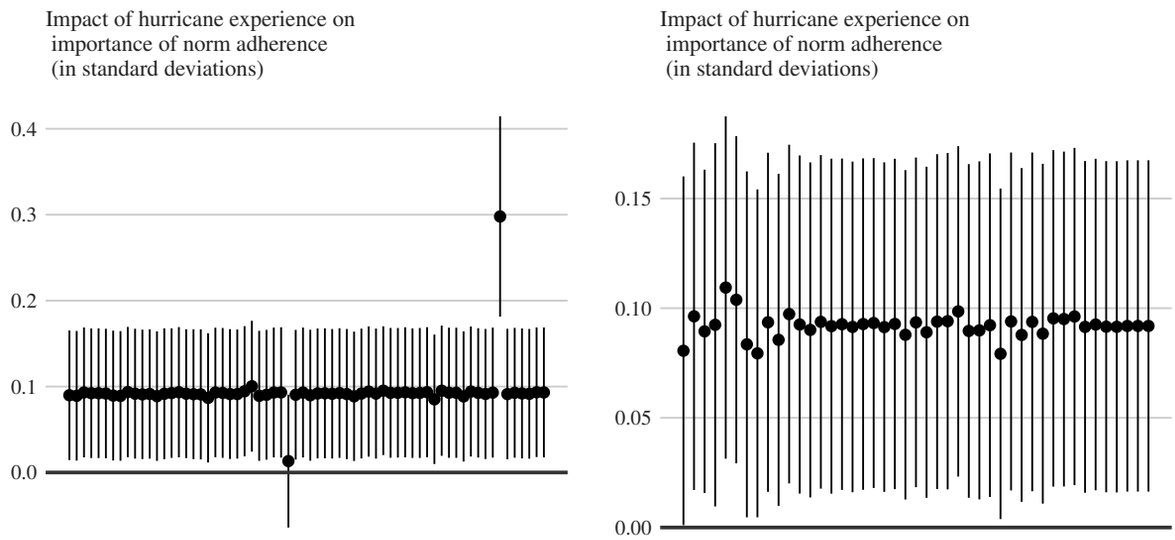
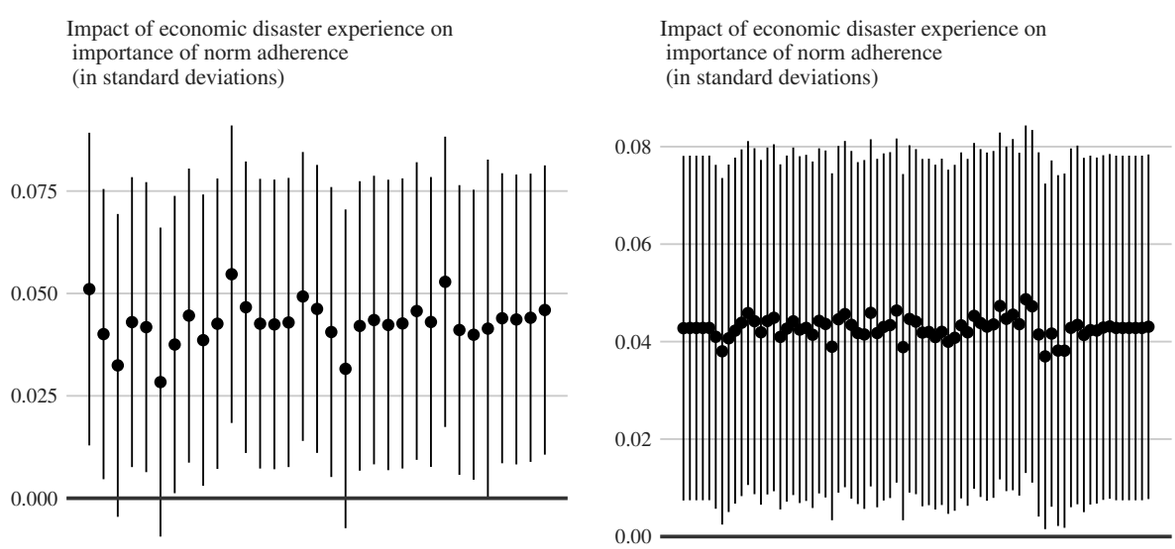


Figure A.12: Robustness of economic disaster results to dropping one country or cohort at a time.



B Appendix to Chapter 2

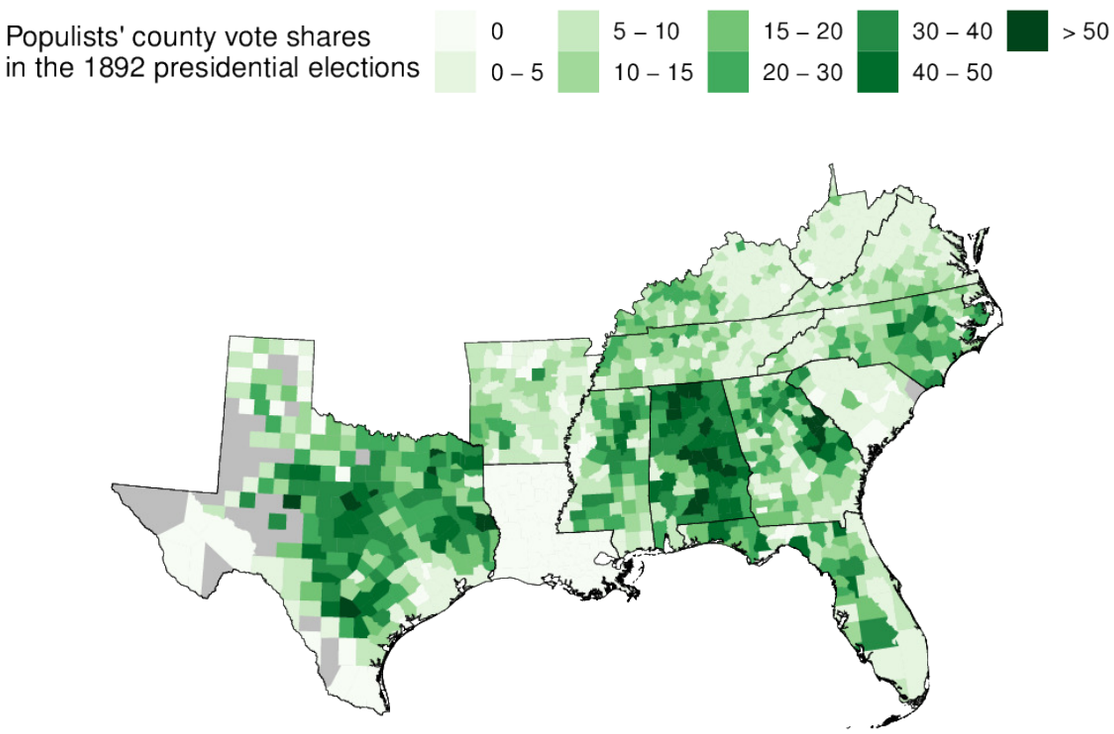


Figure B.1: Populist Party's vote shares across counties in the 1892 presidential election

Notes: This map shows the county-level vote share for the Populist Party in the 1892 presidential election in the U.S. South. Darker greens indicate higher vote shares.

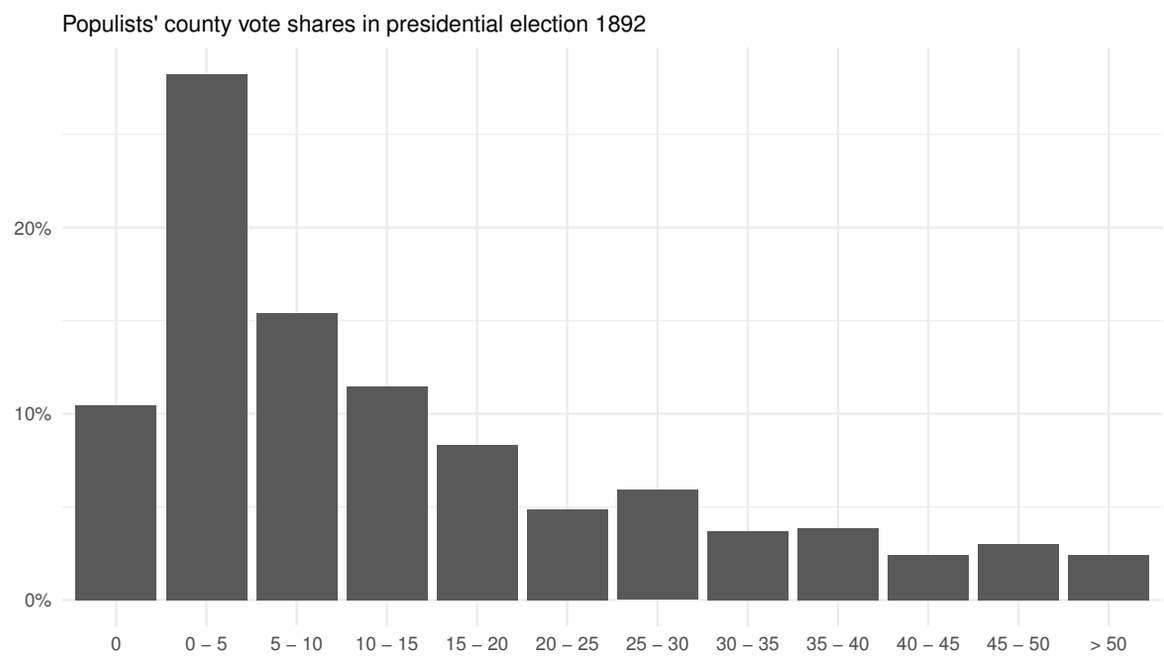


Figure B.2: Distribution of Populist Party's vote share in the 1892 presidential election

Notes: The graph shows the distribution of the Populist Party's vote share in the presidential election of 1892 in the U.S. South.

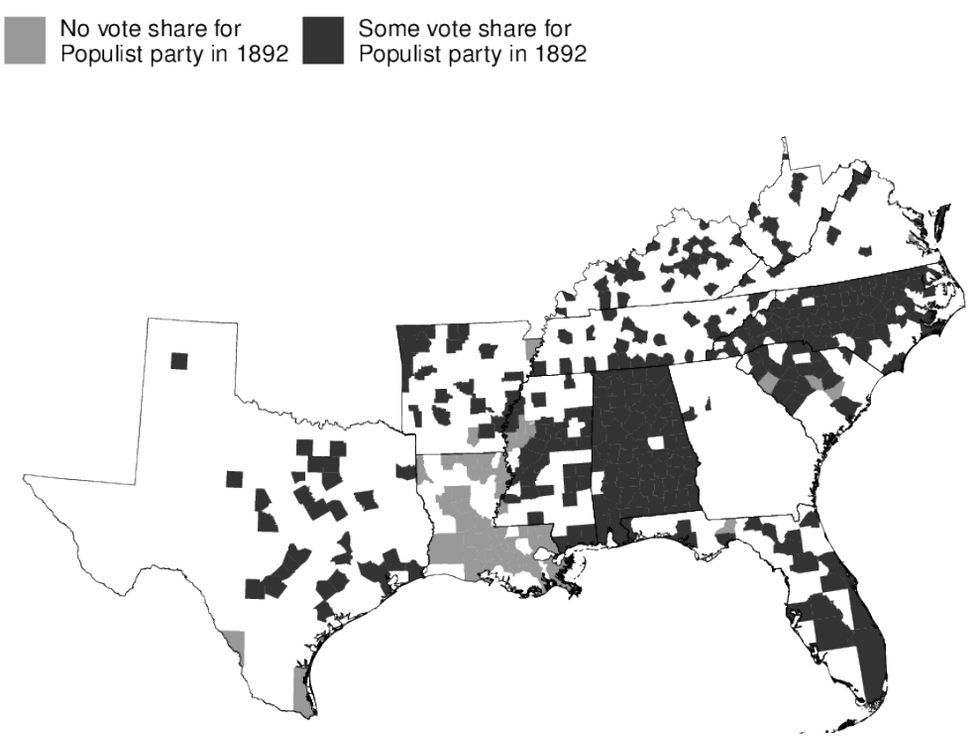


Figure B.3: Distribution of the political threat dummy

Notes: The graph shows the Southern United States in the borders of 1890. Counties in dark or light grey have newspapers in the database and are part of the analysis. Dark (light) grey indicates that the Populist party won some (no) vote share in the 1892 presidential elections.

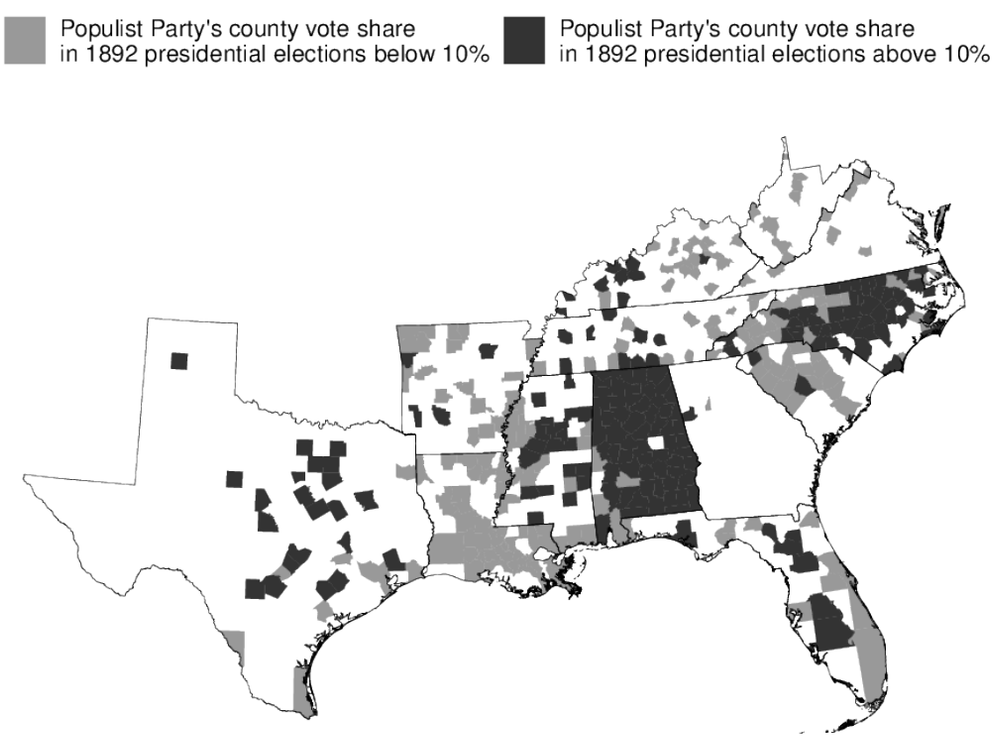
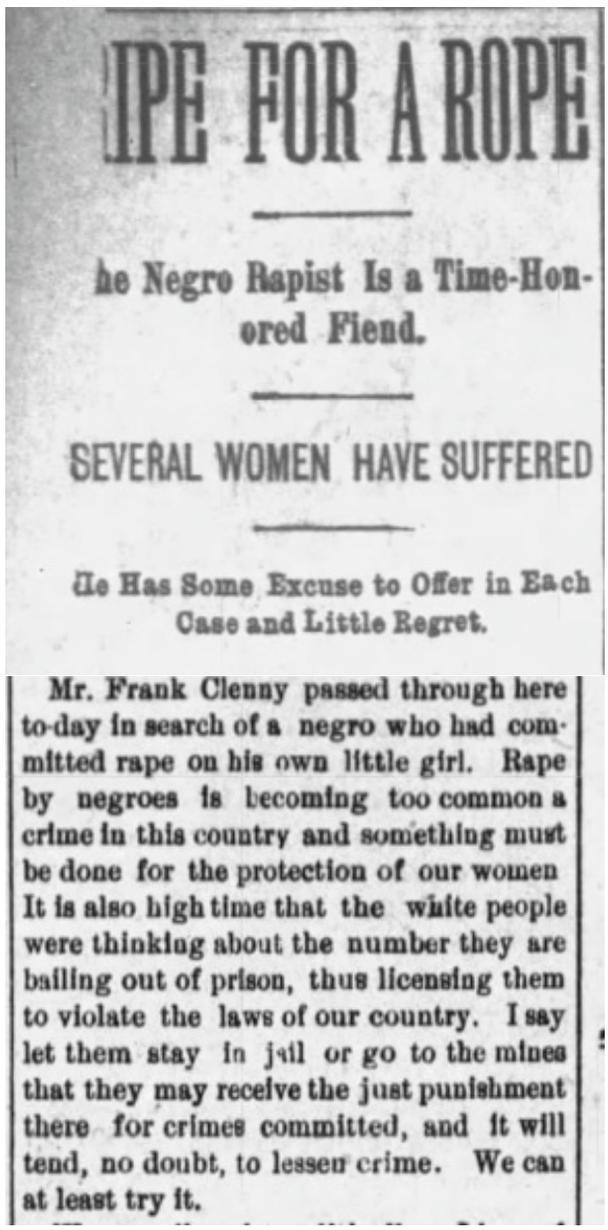


Figure B.4: Distribution of the alternative political threat dummy

Notes: The graph shows the Southern United States in the borders of 1890. Counties in dark or light grey have newspapers in the database and are part of the analysis. Dark (light) grey indicates that the Populist party won a vote share higher (smaller) than 10% in the 1892 presidential elections.



IPE FOR A ROPE

he Negro Rapist Is a Time-Honored Fiend.

SEVERAL WOMEN HAVE SUFFERED

He Has Some Excuse to Offer in Each Case and Little Regret.

Mr. Frank Clenny passed through here to-day in search of a negro who had committed rape on his own little girl. Rape by negroes is becoming too common a crime in this country and something must be done for the protection of our women It is also high time that the white people were thinking about the number they are bailing out of prison, thus licensing them to violate the laws of our country. I say let them stay in jail or go to the mines that they may receive the just punishment there for crimes committed, and it will tend, no doubt, to lessen crime. We can at least try it.

Figure B.5: Illustration of newspaper articles associating African Americans with rapes

Notes: Top panel: Public Ledger, Memphis. Bottom panel: Eufaula Daily Times, 1893

Table B.1: Summary statistics

Statistic	N	Mean	St. Dev.	Min	Max
Anti-Black propaganda	8,086	1.002	1.437	0	22
Political threat indicator	8,086	0.832	0.374	0	1
Post 1892 indicator	8,086	0.597	0.491	0	1
Log population	7,947	10.726	0.595	7.437	13.090
Share black population	7,947	0.397	0.231	0.002	0.934
Residentail segregation	7,790	0.342	0.121	−0.002	0.708
Avg. log p.c. manuf. output	7,782	3.505	1.401	0.000	6.447
Avg. log p.c. farm output	7,947	4.225	0.672	1.074	5.609
Avg. farm size	7,947	144.390	505.320	38	25,576
Rail miles / county sq. miles	7,947	0.526	0.323	0.000	1.877
Avg. indebtedness of farms	7,839	0.447	0.136	0.100	1.000
Avg. interest rate on farm mortgages	7,839	3.650	1.299	0.778	8.221
Share cotton acerage	7,936	0.095	0.094	0.000	0.459
Share share-cropping farms	7,947	0.241	0.131	0.000	0.795
Democrat vote share 1892	8,086	59.563	17.081	21.100	100.000
Democrat newspaper indicator	6,051	0.830	0.376	0.000	1.000

Table B.2: Robustness: Political threat if Populist vote share is great than 10%

	Anti-black propaganda [std.]				
	All	Non Dem.	Democratic Newspapers		
	(1)	(2)	(3)	(4)	(5)
Political threat × Post 1892 [std.]	0.124***	−0.006	0.134***	0.131***	0.102**
	(0.038)	(0.085)	(0.041)	(0.040)	(0.042)
No. of newspapers	764	110	654	654	654
Newspaper FE	Yes	Yes	Yes	Yes	Yes
Year-region FE	Yes	Yes	Yes	Yes	Yes
Economic conditions × year FE	No	No	No	Yes	Yes
Dem. vote share × year FE	No	No	No	No	Yes
Observations	5,399	666	4,733	4,733	4,733
R^2	0.503	0.602	0.497	0.525	0.530

Notes: The table shows that the main result replicates if we define political threat as an indicator equal to one if the Populist Party gained more than 10% of the vote share in the newspaper's county in the presidential election of 1892. An observation is a newspaper-year from 1885 to 1903. The outcome in each column is anti-Black propaganda in newspapers. All regressions include newspaper and year × census region fixed effects. Column 1 shows the estimate for the full sample. Column 2 restricts the sample to newspapers that do not endorse the Democratic Party. Column 3 to 5 restricts the sample to newspapers that endorse the Democratic Party or for which this information is not available. Column 4 adds controls for county-level economic conditions in 1890, interacted with year dummies. These controls are described in Table 1. Column 5 adds controls for the vote shares for the Democratic Party in the 1892 presidential election, interacted with year dummies. All variables are standardized to z-scores. The standard errors are clustered on counties and reported in parentheses. ***, **, and * indicate significance at 1, 5, and 10 % levels.

Table B.3: Robustness: Political threat dummy and Populist vote share.

	Anti-black propaganda [std.]				
	All	Non Dem.	Democratic Newspapers		
	(1)	(2)	(3)	(4)	(5)
Political threat × Post 1892 [std.]	0.127***	−0.016	0.149***	0.180***	0.165***
	(0.047)	(0.107)	(0.050)	(0.052)	(0.054)
Populist vote share × Post 1892 [std.]	0.063	0.054	0.058	0.057	0.014
	(0.048)	(0.135)	(0.051)	(0.051)	(0.055)
No. of newspapers	764	110	654	654	654
Newspaper FE	Yes	Yes	Yes	Yes	Yes
Year-region FE	Yes	Yes	Yes	Yes	Yes
Economic conditions × year FE	No	No	No	Yes	Yes
Dem. vote share × year FE	No	No	No	No	Yes
Observations	5,399	666	4,733	4,733	4,733
R^2	0.502	0.602	0.496	0.526	0.530

Notes: The table shows that the main result replicates if we add the vote share for the Populist Party in the presidential election of 1892 to the regression. An observation is a newspaper-year from 1885 to 1903. The outcome in each column is anti-Black propaganda in newspapers. The main independent variable is an indicator equal to one if the Populist Party gained votes in the presidential election of 1892 in the newspaper's county (first difference) interacted with an indicator equal to one for years greater than 1892 (second difference). All regressions include newspaper and year-census region fixed effects. Column 1 shows the estimate for the full sample. Column 2 restricts the sample to newspapers that do not endorse the Democratic Party Column 3 to 5 focus on newspapers that endorse the Democratic Party of for which this information is not available. Column 4 adds county-level economic controls, measured in 1890, and interacted with year dummies. These controls are described in Table 1. Column 5 adds controls for the vote shares for the Democratic Party in the 1892 presidential election, interacted with year dummies. All variables are standardized to z-scores. The standard errors are clustered on counties and reported in parentheses. ***, **, and * indicate significance at 1, 5, and 10 % levels.

Table B.4: Robustness: Quintiles of Populist vote share

	Anti-black propaganda [std.]				
	All	Non Dem.	Democratic Newspapers		
	(1)	(2)	(3)	(4)	(5)
2nd quintile Political threat × Post 1892 [std.]	0.013	−0.097	0.027	0.056	0.049
	(0.042)	(0.072)	(0.044)	(0.041)	(0.041)
3rd quintile Political threat × Post 1892 [std.]	0.107***	0.047	0.118***	0.137***	0.128***
	(0.031)	(0.079)	(0.032)	(0.035)	(0.036)
4th quintile Political threat × Post 1892 [std.]	0.158***	0.011	0.178***	0.188***	0.169***
	(0.034)	(0.066)	(0.038)	(0.038)	(0.042)
5th quintile Political threat × Post 1892 [std.]	0.140***	−0.013	0.161***	0.189***	0.154***
	(0.044)	(0.075)	(0.049)	(0.053)	(0.059)
No. of newspapers	764	110	654	654	654
Newspaper FE	Yes	Yes	Yes	Yes	Yes
Year-region FE	Yes	Yes	Yes	Yes	Yes
Economic conditions × year FE	No	No	No	Yes	Yes
Dem. vote share × year FE	No	No	No	No	Yes
Observations	5,399	666	4,733	4,733	4,733
R^2	0.504	0.604	0.499	0.527	0.531

Notes: The table shows that the main result replicates for the upper quintiles of the Populist Party vote share in the presidential election of 1892. An observation is a newspaper-year from 1885 to 1903. The outcome in each column is anti-Black propaganda in newspapers. The main independent variables are indicators equal to one for the 2nd, 3rd, 4th, and 5th quintile of the vote share for Populist Party in the newspaper's county in the presidential election of 1892 (first difference) interacted with an indicator equal to one for years greater than 1892 (second difference). All regressions include newspaper and year × census region fixed effects. Column 1 shows the estimate for the full sample. Column 2 restricts the sample to newspapers that do not endorse the Democratic Party. Column 3 to 5 restricts the sample to newspapers that endorse the Democratic Party or for which this information is not available. Column 4 adds controls for county-level economic conditions in 1890, interacted with year dummies. These controls are described in Table 1. Column 5 adds controls for the vote shares for the Democratic Party in the presidential elections of 1892, interacted with year dummies. All variables are standardized to z-scores. The standard errors are clustered on counties and reported in parentheses. ***, **, and * indicate significance at 1, 5, and 10 % levels.

Table B.5: Robustness: Balanced panel.

	Anti-black propaganda [std.]			
	All	Democratic Newspapers		
	(1)	(2)	(3)	(4)
Political threat × Post 1892 [std.]	0.156***	0.233***	0.310***	0.271***
	(0.041)	(0.071)	(0.089)	(0.088)
No. of newspapers	60	54	54	54
Newspaper FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	No
Economic conditions × year FE	No	No	Yes	Yes
Dem. vote share × year FE	No	No	No	Yes
Observations	5,399	1,026	1,026	1,026
R^2	0.502	0.529	0.621	0.637

Notes: The table shows that the main result replicates if we restrict the sample to the balanced panel. An observation is a newspaper-year from 1885 to 1903. The outcome in each column is anti-Black propaganda. The main independent variable is an indicator equal to one if the Populist Party gained votes in the newspaper's county in the presidential election of 1892 (first difference) interacted with an indicator equal to one for years greater than 1892 (second difference). All regressions include newspaper and year × census region fixed effects. Column 1 shows the estimate for the full sample. Column 2 to 4 focus on newspapers that endorse the Democratic Party of for which this information is not available. Column 3 adds controls for county-level economic conditions in 1890, interacted with year dummies. These controls are described in Table 1. Column 4 adds controls for the vote shares for the Democratic Party in the presidential elections of 1892, interacted with year dummies. All variables are standardized to z-scores. The standard errors are clustered on counties and reported in parentheses. ***, **, and * indicate significance at 1, 5, and 10 % levels.

Table B.6: Robustness: Dropping newspapers in Louisiana

	Anti-black propaganda [std.]				
	All	Non Dem.	Democratic Newspapers		
	(1)	(2)	(3)	(4)	(5)
Political threat × Post 1892 [std.]	0.156***	0.033	0.178***	0.161***	0.138**
	(0.046)	(0.095)	(0.052)	(0.052)	(0.058)
No. of newspapers	710	104	606	606	606
Newspaper FE	Yes	Yes	Yes	Yes	Yes
Year-region FE	Yes	Yes	Yes	Yes	Yes
Economic conditions × year FE	No	No	No	Yes	Yes
Dem. vote share × year FE	No	No	No	No	Yes
Observations	4,914	615	4,299	4,299	4,299
R^2	0.508	0.613	0.501	0.533	0.537

Notes: The table shows that the main result replicates if we drop newspapers from Louisiana. An observation is a newspaper-year from 1885 to 1903. The outcome in each column is anti-Black propaganda in newspapers. The main independent variable is an indicator equal to one if the Populist Party gained votes in the presidential election of 1892 in the newspaper's county (first difference) interacted with an indicator equal to one for years greater than 1892 (second difference). All regressions include newspaper and year × census region fixed effects. Column 1 shows the estimate for the full sample. Column 2 restricts the sample to newspapers that do not endorse the Democratic Party Column 3 to 5 focus on newspapers that endorse the Democratic Party of for which this information is not available. Column 4 adds county-level economic controls, measured in 1890, and interacted with year dummies. These controls are described in Table 1. Column 5 adds controls for the vote shares for the Democratic Party in the 1892 presidential election, interacted with year dummies. All variables are standardized to z-scores. The standard errors are clustered on counties and reported in parentheses. ***, **, and * indicate significance at 1, 5, and 10 % levels.

Table B.7: Placebo test: political threat and rape unrelated to African Americans

	White rape terminology [std.]				
	All	Non Dem.	Democratic Newspapers		
	(1)	(2)	(3)	(4)	(5)
Political threat × Post 1892 [std.]	−0.034	−0.045	−0.026	−0.010	−0.049
	(0.053)	(0.218)	(0.056)	(0.057)	(0.066)
No. of newspapers	764	110	654	654	654
Newspaper FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	No	No
Region-year FE	No	No	No	Yes	Yes
Economic conditions × year FE	No	No	No	No	Yes
Dem. vote share × year FE	No	No	No	No	No
Observations	5,387	666	4,721	4,721	4,721
R^2	0.316	0.379	0.315	0.352	0.357

Notes: The table shows that political threat due to the rise of the Populist Party does not affect the frequency of rape terminology unrelated to African Americans. An observation is a newspaper-year from 1885 to 1903. The outcome in each column is the frequency of rape terminology in newspapers, net of anti-Black propaganda in newspapers. The main independent variable is an indicator equal to one if the Populist Party gained votes in the newspaper's county in the presidential election of 1892 (first difference) interacted with an indicator equal to one for years greater than 1892 (second difference). All regressions include newspaper and year × census region fixed effects. Column 1 shows the estimate for the full sample. Column 2 restricts the sample to newspapers that do not endorse the Democratic Party. Column 3 to 5 restricts the sample to newspapers that endorse the Democratic Party. Column 4 adds controls for county-level economic conditions in 1890, interacted with year dummies. These controls are described in Table 1. Column 5 adds controls for the vote shares for the Democratic Party in the 1892 presidential election, interacted with year dummies. All variables are standardized to z-scores. The standard errors are clustered on counties and reported in parentheses. ***, **, and * indicate significance at 1, 5, and 10 % levels.

C Appendix to Chapter 3

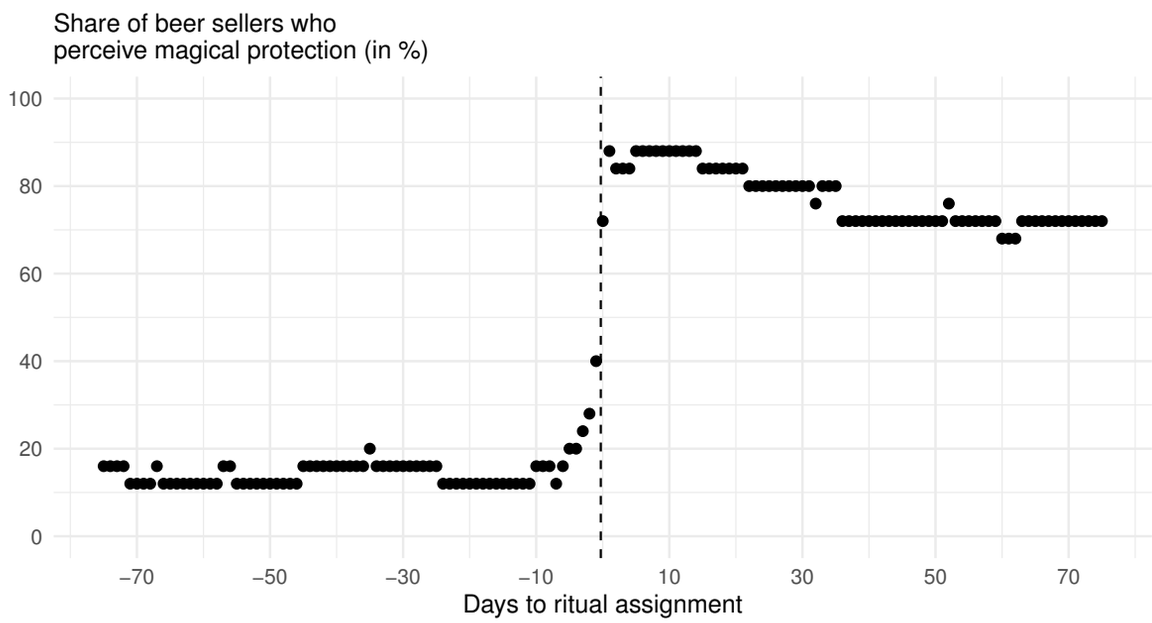


Figure C.1: Effect of treatment assignment on perceptions of protection

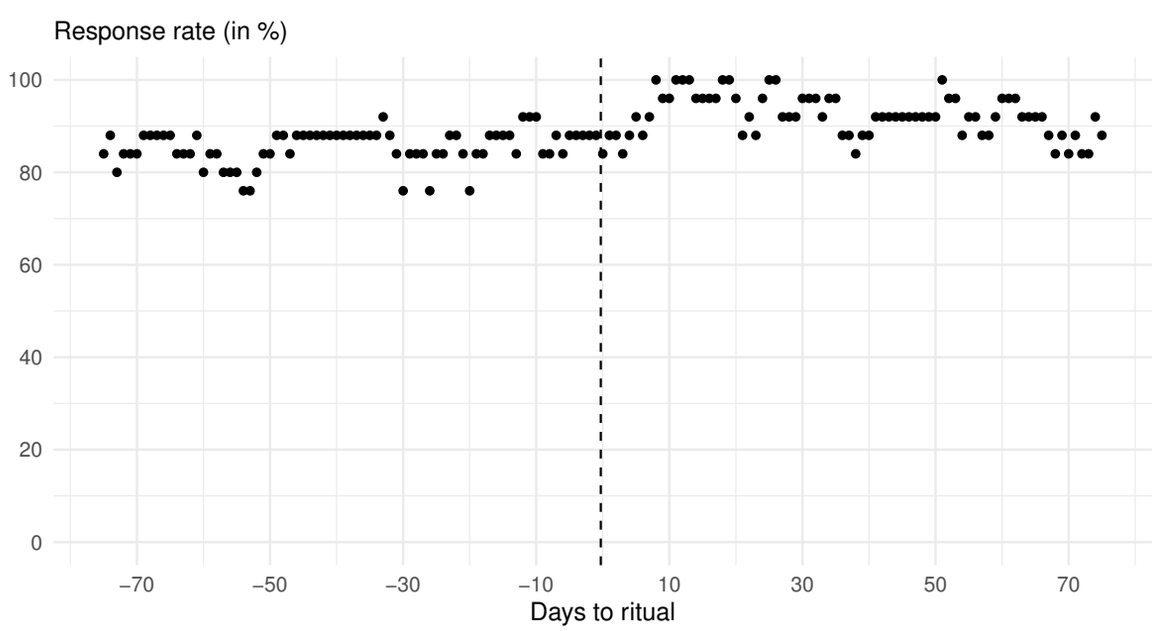


Figure C.2: Response rate around treatment

Table C.1: Summary statistics of main variables

Statistic	N	Mean	St. Dev.	Min	Max
Ritual assigned	918	0.492	0.500	0	1
Ritual received	918	0.495	0.500	0	1
Revenues (USD)	918	18.240	27.322	0	295.996
Number of bottles sold	918	14.125	21.769	0	215
Number of bottles purchased	918	18.219	80.883	0	1,380
Stock-out indicator	918	0.046	0.209	0	1
Closing time	908	20.393	1.343	16.050	25.067
Feeling protected indicator	910	0.322	0.467	0	1
Theft expectation indicator	918	0.101	0.302	0	1
Theft indicator	918	0.020	0.139	0	1
Stress level	918	1.408	2.480	0	10
Post-treatment-theft indicator	918	0.093	0.290	0	1
Sale Prices (USD)	3,157	1.232	0.585	0.233	3.333

Notes: The sample covers 27 days from July 17th to August 13th, 2017 for 35 beer retailers who report consistent data during the sample period. *Ritual assigned* takes value 1 during days on and following the ritual assignment, 0 otherwise. *Ritual received* takes value 1 during days on and following the ritual actually received, 0 otherwise. *Stock-out indicator* takes value 1 if the inventory reaches zero during the day, 0 otherwise. *Closing time* is the hour of the day beer sellers close the shop, where we recode past midnight hours such that increasing values indicate later closing hours (i.e., 1am becomes 25). *Feeling protected indicator* takes value 1 during days when retailers indicate the magical protection works, 0 otherwise. *Theft expectation indicator* takes value 1 during days when beer sellers expect a theft incident during the next seven days, 0 otherwise. *Theft indicator* takes value 1 during days when someone stole inventory from beer sellers, 0 otherwise. *Stress level* is self-reported, ranging from 0 to 10, with increasing values indicating higher stress level. *Post-treatment theft* takes value 1 during days following a theft incidence after beer sellers received the ritual, 0 otherwise.

Table C.2: pre-determined characteristics

Pre-study outcome	Mean	Estimate	Std. error	p-value
Male	0.854	0.252	0.219	0.250
Revenues (USD)	13.328	-14.716	8.236	0.074
Number of bottles sold	11.353	-10.770	6.866	0.117
Number of bottles purchased	9.608	-9.751	6.831	0.154
Stock-out indicator	0.050	0.037	0.045	0.406
Closing time	20.350	-0.176	0.436	0.687
Theft expectation indicator	0.016	-0.003	0.013	0.792
Theft indicator	0.074	0.078	0.056	0.166
Stress level	1.643	1.241	0.713	0.082

Notes: The table presents coefficients for 9 OLS regressions of pre-determined characteristics on an indicator equal to one if the beer retailer reports being protected for at least one day in the sample period. The characteristics include beer sellers' gender and the outcome variables of the analysis but measured between June 1st to July 15th, 2017, before our sampling period for the main analysis. Robust standard errors in parentheses clustered at the beer retailer level. The F statistic of the null hypothesis that all independent variables of a regression of the indicator on the 9 covariates are jointly zero is 17.12 ($p < 0.001$)

Table C.3: Main results: intent-to-treat and IV analyses of beer and non-beer products

	Revenues beers (USD)		Number of beer bottles sold		Revenues non-beers (USD)		Number of non-beer bottles sold	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
				Panel A. Baseline				
Intent-to-treat	2.310	2.843	1.149	1.736	0.707	0.986	0.749	1.073
Beta coefficient	0.057	0.070	0.036	0.054	0.095	0.133	0.066	0.095
IV	2.460	3.158	1.223	1.929	0.753	1.095	0.798	1.191
p value for no effect	*0.006*	*0.004*	*0.009*	*0.008*	*0.001*	*0.001*	*0.007*	*0.011*
				Panel B. Post-Treatment Theft				
Intent-to-treat	2.777	2.637	1.578	1.552	0.778	0.960	0.855	1.033
Beta coefficient	0.068	0.065	0.049	0.048	0.105	0.130	0.076	0.091
IV	2.963	2.930	1.684	1.724	0.830	1.067	0.912	1.148
p value for no effect	*0.009*	*0.012*	*0.013*	*0.022*	*0.000*	*0.001*	*0.008*	*0.013*
Post-treatment theft (ITT)	−5.600	−6.252	−5.148	−5.610	−0.855	−0.777	−1.264	−1.199
Beta coefficient	−0.090	−0.101	−0.105	−0.114	−0.076	−0.069	−0.073	−0.070
Post-treatment theft (IV)	−5.666	−6.213	−5.186	−5.587	−0.873	−0.763	−1.284	−1.184
p value for no effect	*0.134*	*0.121*	*0.126*	*0.076*	*0.065*	*0.076*	*0.093*	*0.112*
Beer seller fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Week-of-year fixed effects	No	Yes	No	Yes	No	Yes	No	Yes
Day-of-week fixed effects	No	Yes	No	Yes	No	Yes	No	Yes
Mean (std. dev.) dep. var.	12.02 (20.36)		8.54 (16.13)		2.33 (3.71)		3.28 (5.65)	
Observations	645	645	645	645	645	645	645	645

Notes: This table presents intent-to-treat and IV estimates of the effects of the ritual on revenues and quantities sold of beer and non-beer products. The unit of observation is beer seller by day. The dependent variable in columns 1 and 2 is revenues from beer products (converted to USD); in columns 3 and 4, it is number of beer bottles sold; in columns 5 and 6, it is revenues from non-beer products (converted to USD); and in columns 7 and 8, it is number of non-beer bottles sold. All regressions include beer seller fixed effects. Columns 2, 4, 6, and 8 add day-of-week and week-of-year fixed effects. For each regression, intent-to-treat coefficients, beta coefficients of the ITT, IV coefficients, and p-values for no effect (in italics) are reported. p-values are computed using randomization inference with 10,000 reassignments of treatment status. All regressions are weighted by the inverse probability of being treated.

Table C.4: Placebo results for non-believer sample: business outcomes

	Revenues (USD)		Number of bottles sold		Sale Price (USD)	
	(1)	(2)	(3)	(4)	(5)	(6)
Intent-to-treat	−11.251	−8.433	−8.844	−6.596	0.047	0.005
Beta coefficient	−0.165	−0.124	−0.178	−0.133	0.043	0.004
IV	−11.305	−9.306	−8.887	−7.279	0.047	0.005
p value for no effect	*0.311*	*0.327*	*0.299*	*0.326*	*0.576*	*0.579*
Beer seller fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Beer brand fixed effects	No	No	No	No	Yes	Yes
Week-of-year fixed effects	No	Yes	No	Yes	No	Yes
Day-of-week fixed effects	No	Yes	No	Yes	No	Yes
Mean (std. dev.) dep. var.	26.88 (34.05)		19.04 (24.82)		1.33 (0.54)	
Observations	136	136	136	136	498	498

Notes: This table presents intent-to-treat and IV estimates of the effects of the ritual on business outcomes for the sample of non-believing beer sellers. The dependent variable in columns 1 and 2 is revenues (converted to USD); in columns 3 and 4, it is number of bottles sold; in columns 5 and 6, it is sale prices (converted to USD). In columns 1 to 4, the unit of observation is beer seller by day; in columns 5 and 6, it is beer brand by beer seller by day. All regressions include beer seller fixed effects. Columns 2, 4, and 6 add day-of-week and week-of-year fixed effects; and columns 5 and 6 also include brand fixed effects. For each regression, intent-to-treat coefficients, beta coefficients of the ITT, IV coefficients, and p-values for no effect (in italics) are reported. p-values are computed using randomization inference with 10,000 reassignments of treatment status. All regressions are weighted by the inverse probability of being treated.

Table C.5: Placebo results for non-believer sample: purchases, stock-outs, and closing times

	Number of bottles purchased		Stock-out indicator		Closing time	
	(1)	(2)	(3)	(4)	(5)	(6)
Intent-to-treat	−36.581	−15.851	0.002	−0.018	0.009	−0.070
Beta coefficient	−0.234	−0.101	0.006	−0.060	0.004	−0.029
IV	−36.758	−17.491	0.002	−0.020	0.009	−0.078
p value for no effect	*0.028*	*0.036*	*0.633*	*0.302*	*0.686*	*0.287*
Beer seller fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Week-of-year fixed effects	No	Yes	No	Yes	No	Yes
Day-of-week fixed effects	No	Yes	No	Yes	No	Yes
Mean (std. dev.) dep. var.	23.46 (78.19)		0.02 (0.15)		20.65 (1.23)	
Observations	136	136	136	136	134	134

Notes: This table presents intent-to-treat and IV estimates of the effects of the ritual on purchases, stock-outs and closing times for the sample of non-believing beer sellers. The unit of observation is beer seller by day. The dependent variable in columns 1 and 2 is number of bottles purchased; in columns 3 and 4, it is an indicator variable that equals one if beer sellers stock was zero; in columns 5 and 6, it is the time when beer sellers closed the shop. All regressions include beer seller fixed effects. Columns 2, 4, and 6 add day-of-week and week-of-year fixed effects. For each regression, intent-to-treat coefficients, beta coefficients of the ITT, IV coefficients, and p-values for no effect (in italics) are reported. p-values are computed using randomization inference with 10,000 reassignments of treatment status. All regressions are weighted by the inverse probability of being treated.

Table C.6: Placebo results for non-believer sample: perceptions, expectations, and emotions

	Feeling protected indicator		Theft expectation indicator		Theft indicator		Stress level [0-10]	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Intent-to-treat	0.000	0.000	0.047	0.051	0.000	0.009	0.291	0.517
Beta coefficient	0.000	0.000	0.273	0.299	−0.000	0.037	0.056	0.100
IV			0.047	0.057	0.000	0.010	0.293	0.570
p value for no effect	*1.000*	*1.000*	*0.108*	*0.091*	*0.941*	*0.251*	*0.254*	*0.212*
Beer seller fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Week-of-year fixed effects	No	Yes	No	Yes	No	Yes	No	Yes
Day-of-week fixed effects	No	Yes	No	Yes	No	Yes	No	Yes
Mean (std. dev.) dep. var.	0.00 (0.00)		0.01 (0.09)		0.01 (0.12)		1.40 (2.58)	
Observations	136	136	136	136	136	136	136	136

Notes: This table presents intent-to-treat and IV estimates of the effects of the ritual on perceptions, theft expectations, actual theft, and perceived stress for the sample of non-believing beer sellers. The unit of observation is beer seller by day. The dependent variable in columns 1 and 2 is an indicator variable equal to one if beer seller feels protected from theft; in columns 3 and 4, it is an indicator variable equal to one if beer seller expects a theft incidence in the following week; in columns 5 and 6, it is an indicator variable equal to one if retailer experienced a theft incidence during the day; and in columns 7 and 8, it is self-reported stress level. All regressions include beer seller fixed effects. Columns 2, 4, 6, and 8 add day-of-week and week-of-year fixed effects. For each regression, intent-to-treat coefficients, beta coefficients of the ITT, IV coefficients, and p-values for no effect (in italics) are reported. p-values are computed using randomization inference with 10,000 reassignments of treatment status. All regressions are weighted by the inverse probability of being treated.

Part IV

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Part V

Curriculum Vitae

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Professional Experience

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